VOL. LII. NO. 206.] [APRIL, 1943.

MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

I.—KNOWLEDGE OF PHYSICAL OBJECTS.

BY A. C. EWING.

FROM the time of Descartes onwards it has been one of the chief puzzles of philosophers to see how we could justify the claim we ordinarily make to knowledge of the physical world, and it has since then generally been held, even by "realists", that there is some degree of uncertainty attaching to all physical object propositions, however slight it may be. But a modern school of philosophers have proposed at one fell swoop to eliminate all doubts on the subject, not indeed as merely ungrounded, but as meaningless, by laying down the principle that all meaningful statements must be capable of verification or falsification by sense-experience and concluding that, since philosophers have no dispute as to the facts of sense-experience, their disputes are either verbal or meaningless. That the school in question, in so far as it carries out its programme consistently, also throws out a great many babies with the bath-water seems clear to me; but I do not wish to discuss these now, and if the verificationists to whom I am referring have really succeeded in showing that any general doubt as to the existence of physical objects is unmeaning, they will have earned the utmost gratitude from philosophers for removing one of the chief difficulties that for many generations has beset them. What a large proportion of philosophical labour has been expended either in justifying the belief that we have knowledge of "independent" physical objects or in showing that it cannot be justified! And now we

are saved all this labour by the single verification formula, which enables us to say that there is none but a verbal difference between the contending schools, that to assert the existence of physical objects in any sense in which their existence cannot be verified is to talk nonsense, and that to say that we can doubt the existence of physical objects is also meaningless. True, we shall have to content ourselves with a view which does not admit anything in the notion of a physical object beyond a class of propositions about human observations, actual or possible; but since to assert anything else would be meaningless, why worry? Since any alternative suggestion has either no meaning at all or the same meaning as the verificationist suggestion, there is literally nothing about which to worry. However, there is a fatal snag in such a comforting view: the verificationist principle on which it is based cannot be proved and would in fact contradict itself if it were true, for it could not be verified itself and so would on its own showing be meaningless, and a statement cannot be both meaningless and true.

As a matter of fact the full-blooded verificationism of which I have just been speaking, and which I criticized in an earlier article in this periodical,[1] has now commonly been abandoned in favour of what might be called methodological verificationism, a way of approach which is not committed to the truth of the verification principle but treats the latter rather as a slogan suggesting the desirability of asking about all assertions—if true, how could they be verified, than as an established proposition. Such a method of procedure may be useful for some purposes; but it must be remembered that, unless we hold the verification principle to be not a methodological fiction but a *true* proposition, we are not justified in accepting the truth of any proposition because it has been deduced by an argument in which the verification principle was a premiss. That it can be deduced from a premiss which is not true is no justification of any proposition. We cannot rule out the possibility of doubts about physical objects by a straightforward deduction from the verification principle unless we are justified in thinking the latter itself is true and not merely methodologically useful. But it may still well be the case that the conclusion that any general doubt about physical objects is meaningless is true, though the verification principle from which it has sometimes been deduced is false. This view indeed attracts me much more than most other points in the verificationist philosophy. Only, if it is

[1] *Vide* my article in Vol. XLVI, pp. 347 ff.

true, we shall have to find other arguments for it than any that could be based on the assertion that all meaningful statements must be verifiable by sense-experience. Having considered such an argument I shall turn to another type of view which involves the same conviction in the certainty of physical object propositions without, however, asserting that denial or doubt is *meaningless*.

People sometimes assume that the doubt can be dismissed just because it "makes no practical difference"; but it may be retorted that, if a person is really interested only in questions which do make a practical difference in the ordinary, narrow sense of "practical", he is not the sort of person who should study philosophy at all, because it cannot possibly interest him. I should have thought it had been believed for centuries that philosophy could not help one to predict particular physical events or psychological experiences; but I cannot see any ground for concluding from this belief (which I share) that "philosophical assertions" are either merely verbal or meaningless, unless we assume the verification principle to be true, an assumption which I have rejected. That philosophical study may have an indirect practical effect of a different sort surely cannot be denied, for obviously it can affect one's general attitude to life and therefore one's emotional reactions and practical decisions: many of the most important events in history on most recognized criteria of importance have been partly determined by general ideas about religion, ethics, or politics which, though not mainly the fruit of philosophical reflection, were influenced, at least indirectly, by the latter (though usually not nearly so much as in my opinion would have been desirable). It may be retorted that theories about the grounds for holding that physical objects exist are not, like ideas about God, going to cause any revolutionary changes in human life and society; but, since all parts of philosophy are liable to reveal unexpected interconnections, we can never be sure that philosophical considerations which originated in some sphere of thought as far removed from the ordinary thoughts of practical life as are the questions which I am discussing in this article may not indirectly affect men's views on subjects of practical importance by suggesting and supporting a particular mode of thought which may be applied in other fields. Just as it has been commonly said that the scientific discoveries which have been of most enormous practical importance have usually originated from lines of research which seemed to the researcher incapable of yielding any practical effects at all, so in philosophy we have

XUM

no right to assume that ideas of practical importance may not emerge from or be affected by studies in fields that appear to have no practical bearing whatever. Most of the great philosophers of the past have thought it worth their while to devote very careful attention indeed to the theory of physical objects and the theory of knowledge; and since they would at least claim that their philosophy is a systematically interconnected whole, it follows that considerations arising from these fields are likely to have influenced other parts of their philosophical teaching more practical in their relevance. To look too closely for practical results in one's philosophy and aim too directly at them is not the best way of obtaining them. Philosophy cannot indeed be practical in the sense in which the art of making shoes or the science of physiology is practical; but if "practical" means "relevant to human happiness and misery" there is hardly anything more practically important than have been certain philosophical or quasi-philosophical ideas.

The verificationist, whether dogmatic or methodological, has at least shown one thing, namely, that the general doubt about the existence of physical objects is a different kind of doubt from the doubt about the existence of particular objects when there are particular reasons for doubting it. The philosophical doubt about physical objects differs from such doubts, which occur at the level of common sense, in four respects. (1) It is not an emotional but only a theoretical doubt: the philosopher *feels* quite certain about what he says he doubts, at least when considering it outside his study in the hurly-burly of practical life and probably even in most philosophical discussions themselves. (2) It has no *direct* practical effect, *i.e.* philosophers who profess the doubt are just as keen to avoid moving motor-cars and bombs and to eat food as are philosophers who do not profess it or ordinary practical men. (3) It is not based on the same kind of grounds as ordinary mundane doubts, *e.g.* doubts as to whether the lake a man seems to see in a desert is not really only a mirage. (4) It is characterized by extreme generality —if it affects any, it affects all physical object propositions. But to say that it is a different kind of doubt is not to say that it is no doubt at all. Or, even if we say that human beings are psychologically incapable of doubting all physical object propositions, it may still be true that they are really doubtful and that we can see that they are so, though we cannot hold this proposition in mind for long, apply it in practice or feel as if it were true. Still less does the psychological difficulty of entertaining a doubt make the doubt meaningless.

Now I should myself like to be able to accept the suggestion that the doubt in question is meaningless, for then I should have a clear justification for holding as indubitable many things which almost all philosophers would like to hold indubitable but cannot prove, for if the doubt as to physical objects may be dismissed as meaningless we should be justified in treating in the same way any professed d ıbt as to the existence of other minds than the speaker's own o any suggestion that memory is always untrustworthy. We should then discover a justification for Dr. Moore's view that we know (in the strict sense of " know ") many common-sense propositions about these matters, a view which he holds without having been able to give any argument to justify it except a reference to other common-sense propositions about which the question could equally well have been asked how we know them, as when he proves that external objects exist by merely pointing out that his hands exist.[1] But I cannot get away from the fact that I see quite clearly that the statements that memory is always untrustworthy or that the existence of any physical object is doubtful, whether true or false, have at least a good plain meaning, however hard it may be to furnish a quite adequate philosophical " analysis " of their meaning. I must therefore reject the proffered consolation and face my doubts hoping that they may after all turn out, not indeed meaningless, but unjustified.

However, there is one argument used in favour of the view that such doubts are meaningless which I must first discuss. It is urged that doubt is only significant when there is some particular reason for doubting and some conceivable criteria which could be relevant to that doubt. And even people who do not hold the verificationist principle in general may contend that in the case of physical things at any rate we are justified in saying that the reason for the doubt and the criteria for settling it must be empirical because, whether there are questions which can be settled by other than empirical criteria or not, at any rate empirical criteria are the only criteria which can settle questions about the existence or properties of physical things. Everybody admits that it is not senseless to doubt whether the man in the distance is Smith on the ground that I am looking through a dense mist which distorts what I see, that a person I see in the distance even under normal conditions sometimes turns out on closer inspection not to be the person I first took him to be, and that I have heard from a fairly reliable source

[1] *Proof of an External World*, p. 25.

XUM

that Smith sailed for Australia yesterday. But it is contended that it is senseless to raise the doubt whether anything I see ever is as I see it on the ground that there can be no conceivable empirical criteria which would be relevant to that doubt. There are no possible experiences, it is said, which could cast doubt on all physical object propositions, only on some. You cast doubt on some only by reference to other physical object propositions, and therefore you cannot cast doubt on them all.

In reply I should flatly deny that empirical circumstances cannot be relevant to the general doubt. In fact the philosophers who have cast doubt on our certainty as to physical objects have usually done so on the ground of particular empirical circumstances, *e.g.* dreams or the occurrence of what subsequent experience shows to be "illusions". Now it may be held that the empirical evidence given does not justify their conclusion that all physical object propositions are doubtful. I shall have something to say about that later. But this can at any rate only be settled by an investigation of the particular grounds given and not by a general argument against the possibility of doubt. Further, it is quite conceivable that illusions and dreams might have been much more frequent and much more difficult to distinguish from what we regard as veridical perception than is actually the case. Suppose when we looked at what we took to be the same place we very rarely had an experience in the least similar twice running, suppose it were completely impossible to arrive at any sort of agreement between different men as to the physical things around us, suppose what I took to be the same physical object behaved like water one moment and like steel the next without there being any discernible regularity whatever in these changes, suppose I seemed sometimes to have the body of a toad and sometimes of an elephant and sometimes no body at all. Under these circumstances it is surely plain that a doubt as to the reliability of any apparent perceptions of physical objects would be justified, whether it is or is not justified to-day. Now the occurrence of such circumstances is perfectly possible logically. It may be doubted whether life could have established and maintained itself under such conditions and so as to whether we should have been there to doubt the existence of physical objects; but I am not sure whether any argument against the supposition to that effect does not vitiate itself by already presupposing the existence of a physical world, and in any case it is *logically* possible that life might have existed under such circumstances for anything we can see. And we can surely imagine a state of affairs in which,

though things did not go to the extreme which I have suggested, the reports of different people on things they observed were more difficult to reconcile than they actually are, a state in which illusions were more frequent and more puzzling, a state in which everybody was apt to have hallucinations and dreams without warning in the middle of their waking experience. Consequently, even if we now think that the argument of the sceptic fails so completely as to leave us still in possession of absolutely certain knowledge of physical objects, we can easily imagine circumstances in which the element in our experience that we call "illusion" was so much more prominent and baffling than it is that we should have to admit that the existence of physical objects was more or less doubtful and that no physical object judgements were absolutely certain. Contrariwise, if we are already of the opinion that no physical object propositions can be known with absolute certainty, we can imagine a state of affairs in which illusions, dreams and discrepancies between different observers were much less prominent than they are, and we should then have to admit that under such conditions physical object propositions, if not absolutely certain, would be at least nearer certainty than they are in fact for us. This is sufficient to show that empirical circumstances are relevant to the doubt in question, and so the doubt cannot be dismissed on the ground that they are not. The argument which I have quoted in favour of this dismissal could at the best only apply against a person who said that, even if there never were any dreams or perceptions which our subsequent experience inclined us to describe as illusory, physical object propositions would still all be doubtful, and who raised doubts without giving any empirical evidence in their favour; and I do not think that it would even apply against him, because the apparent inadequacy of any conceivable empirical criteria to establish physical object propositions with certainty is of itself a strong argument against the certainty of the latter apart from any evidence based on illusions, etc.

Could my point be met by admitting that empirical criteria are relevant to the doubt but still denying that it is a meaningful doubt on the ground that empirical criteria, though relevant as increasing or decreasing probability, cannot settle the question asked by the doubter completely and prove with certainty either that physical objects do or that they do not exist? No, clearly not, for if the doubt is meaningless no empirical criteria can possibly even be relevant to the degree of doubtfulness to be attached to the propositions doubted, since it is then senseless

to say that they are doubtful at all. Further, the whole argument rested on the supposition that physical object propositions are propositions of such a kind that their truth could be determined by empirical criteria and no others. Therefore, if it is true that the use of empirical criteria could not under any conceivable circumstances remove the doubt, once it was raised, it follows that physical object propositions are all really uncertain; and on the other hand, if there are conceivable empirical criteria which could remove them, the statement that they are all uncertain may be false but could not be meaningless.

As a matter of fact, as we have seen, the doubt is itself based mainly on empirical circumstances in the sense that they provide premisses from which we may plausibly deduce that the propositions are doubtful. It remains true that our decision should not and could not depend wholly on the empirical evidence taken as brute fact, but on an estimate of the bearing of the latter. Philosophers do agree as to the empirical evidence but differ as to the conclusions they draw from it. This in itself is not peculiar to philosophers; it is often the case also with, *e.g.*, historians, who estimate differently the bearing of acknowledged facts in making their rival theories probable or the reverse. However, it would be unreasonable to deny that there is some difference, hard perhaps to define, between the relation which empirical evidence can bear to philosophical conclusions and the relation which it bears to doubtful historical or other factual conclusions deduced from it. But I do not mean to say that philosophical doubt about physical objects is the same kind of doubt as ordinary doubt about the nature or existence of some particular objects; all I mean to point out is that, if the doubt is dismissed as senseless or not worth considering philosophically on the ground that it would be a doubt to which empirical evidence was irrelevant, the premiss is just not true, so that we need not accept the conclusion even if we granted, which I should not grant, that the truth of the premiss would entail it.

It may be objected that we can only doubt physical object propositions on the strength of other propositions about physical objects, and that therefore it cannot be legitimate to doubt all physical object propositions but only some of them. "How can we know that the flat sense-datum is not part of the surface of the cricket ball, unless we know that there really is a cricket ball and that it really does have a surface which is spherical? Or how do we know that the reflection of the glove is dislocated as to position and reversed as to right and left, unless we know

where the glove itself is and how its parts are arranged?"[1] I should, however, agree with the author from whom I have just quoted in dismissing this objection, though perhaps with somewhat less respect than he shows for it. In the first place, the fact that I *seem* to see the same object on different occasions as possessing qualities which are not compatible with each other might be held sufficient of itself to justify doubt.[2] Assumptions about physical objects play a large part in the argument from illusion as commonly stated; but the argument is not wholly destroyed when these assumptions are taken away. Indeed it may be held that the mere fact that we cannot *prove* the existence of physical objects, if this is a fact, is in view of the impossibility of holding it to be certain that all cases of ostensible perception are cases of direct knowledge sufficient to cast some doubt on all physical object propositions. I am not arguing that the sceptic's argument is successful; that we have to consider later. But at least it is meaningful, and is not in all its forms based on the assumption of the truth of any physical object propositions. But, secondly, I do not think that the fact that physical object propositions formed part of a sceptic's premisses would necessarily vitiate or render uncertain his conclusion in so far as drawn from these premisses. If it can be shown from what he says that a witness contradicts himself this is a good argument for attaching doubt to everything the witness says unless confirmed from other sources, and the sceptic is trying to show that perception is an unreliable witness by showing that some physical object propositions based on perception are incompatible with some of our perceptions. A *reductio ad absurdum* is always recognized as a legitimate form of argument, and a *reductio ad absurdum* consists in showing a view to be false by assuming it to be true and then deducing from this assumption consequences which are absurd or incompatible with the assumption itself or with well-established facts. This is not exactly the argument in question about physical objects, for it is not intended by such arguments to show that the belief in physical objects is logically absurd or even false, but that it is not certain; but the principle is similar. The argument is: even granting the truth of the ordinary belief in the physical world, it follows that the belief still cannot be certain. (And if it is not true, there can be no question of its being

[1] H. H. Price, *Perception*, p. 34.

[2] No doubt it would be logically possible to reply that the object had changed, but this would not remove the doubt raised, for nobody could hold that it was *certain* that the object had changed in this way.

XUM

certain.) The sceptical arguments to which I have referred may not be right, but at least they escape the charge of depending for their validity on the truth of the view which they are intended to assail, except when gratuitously mis-stated by the persons who use them.

Let us now turn to another kind of view which dismisses philosophical doubt in these matters as illegitimate, namely, the view put forward by Dr. G. E. Moore. Without claiming that doubt is meaningless, he does insist that we have certain knowledge of many physical object propositions. In order to maintain this position it is essential, as he recognizes, to distinguish between the proposition that I see it to be logically impossible that p could be false and the proposition that I certainly know p to be true. But in holding that we can know with certainty what it is logically possible might not be true he is in agreement with most philosophers, for most philosophers have maintained that we can know with certainty many propositions about my present state of mind and immediate objects of sense and yet they have not held that these propositions are logically necessary, still less that we could see them to be so. I know now that I am feeling cold, but it is perfectly possible logically, for anything I can see, that I might instead be feeling very hot. Not only does Dr. Moore hold that we can know with certainty what it is logically possible might not be true, but also that we can know with certainty what it is *logically* possible we might be wrong in thinking we knew. (For these two statements are not indistinguishable. It is logically possible that I might not be in existence, but it is not logically possible that I could think I existed and yet not exist.) But again this is quite in accord with the views of most philosophers. Most philosophers, I think, have held that we could by memory know with absolute certainty some propositions about the past (even if other memory-propositions were only believed), although they could not show any logical impossibility to be involved either in the non-happening of the event a man was convinced he remembered or in his thinking he knew it had happened though it had not really done so. As a matter of fact, since, *e.g.*, feeling cold is different from judging that I feel cold, I should hold that there would be no self-contradiction even in the supposition that a man was mistaken in his judgement as to his immediate sense-experience, so that we have even here a case of *knowing* where it is *logically* possible we might be mistaken; but I am not sure whether most philosophers would agree with my view. However, the majority of philosophers have not

when discussing philosophy claimed strict knowledge of propositions about physical objects, as does Dr. Moore.

If we cannot see the denial of a physical object proposition to be self-contradictory, how can we know the proposition to be true? In *Contemporary British Philosophy*[1] Dr. Moore says that we know such propositions without knowing how we know them. In *Proof of an External World*[2] he claims to be able to prove with complete adequacy and certainty that there are external objects by just pointing to some particular physical objects which we can see, but admittedly leaves unanswered the question how we know that these particular objects which provide the premiss of the proof exist and seems to suggest that this question neither admits of nor needs an answer.[3] This position seems to me not satisfactory unless our knowledge here is direct, and so many difficulties have been raised in the course of philosophical controversy against the latter supposition that even in that case a defence against these objections is urgently needed. If we do not know physical object propositions directly—and some of the propositions which Dr. Moore cites as known in *Contemporary British Philosophy* he admits we do not know directly[4]—it is surely incumbent on the philosopher to explain the logical nature of the arguments by which they can be established and show how it is that such arguments yield not probability but complete certainty.

It seems often to be assumed that, if we adopt a phenomenalist view of physical objects and analyse them in terms of the sense-data of human beings, all difficulty in supposing propositions about them to be directly known disappears. But this is by no means the case. For, even if we analyse such propositions in terms of human experience or sense-data, they still go beyond my own sense-data, so that I cannot claim that the content of even the simplest of them is entirely given in my present experience. Even such a statement as "There is a table" includes on the phenomenalist view propositions about the experience or sense-data of other human beings besides myself and about hypothetical observations of my own, *i.e.* it asserts not only that I have table-like sense-data now, but also at least that other human beings would have similar sense-data if they looked under certain conditions and that if I varied the conditions I should have certain other sense-data which I am not having now, for instance, tactual sense-data or visual sense-data obtained from a different point of view. If physical object proposi-

[1] Ed. J. H. Muirhead, *2nd Series*, p. 206.
[2] P. 25 ff. [3] Pp. 29-30. [4] P. 206.

XUM

tions were merely statements about one's present experience and immediate objects of sense, there would be no distinction between propositions about physical objects and propositions about dreams. Therefore, if we claim to know physical object propositions, we are claiming to know something that is not logically necessary and is not given in our present experience, whether we adopt a realist or a phenomenalist view of physical objects. But this claim is not necessarily to be rejected offhand.

In an unpublished paper read at Cambridge several years ago to the Moral Science Club, to which Dr. Moore has kindly given me leave to refer, he defends his position against some stock objections by the sceptic. I have not the paper in front of me but am dependent on a restatement of the argument which I made in a course of lectures that I delivered, so it is very possible that it may not be in all respects a completely accurate account of what Dr. Moore said, *e.g.* I seem to remember that he was concerned more with memory than with physical object propositions in his paper while I have only developed these arguments in regard to physical objects. But I think that it represents adequately arguments that he used, and even irrespective of the question whether he used them the arguments are certainly important and deserve discussion and, if possible, a reply, which in the absence of publication they are hardly likely to obtain from anybody else. Dr. Moore first considers the argument from dreams as used by, *e.g.*, Descartes. This he thinks can be stated in some way like this.

A person who seems to himself to be perceiving a chair may not actually be perceiving one, for he may seem to perceive a chair in what is only a dream.

I am a person who seems to be perceiving a chair now.

Therefore, I may not now be perceiving a chair.

Now, Dr. Moore contends, the first premiss may mean one of two things. It may mean " Some people who seem to themselves to be perceiving a chair are not actually perceiving a chair ". But in that case the argument breaks down because " may " cannot possibly bear that sense in the conclusion. I cannot say, " Some of me is perceiving a chair ". Even if that objection did not hold, he might have added, the argument would in that case commit the fallacy of undistributed middle, being of the form *MoP SaM SoP*. He compares the argument to—A human being may be female, I am a human being, therefore I may be female.

Secondly, he says, the major premiss may mean merely that

it is logically possible that a person who seems to himself to be perceiving a chair is really not perceiving a chair. Then there is no flaw in the argument, but the conclusion is not incompatible with Dr. Moore's view, for he does not hold that it is logically impossible that we could be deceived as regards these physical object propositions but only that we know in some cases that we are not so deceived. As we have seen, he is contending that we can *know* even where it is logically possible that we might be wrong.

I cannot, however, help viewing this reply with considerable suspicion. The difficulty is not merely that some experiences which seem to be instances of perceiving, *e.g.*, chairs are not really so. It is, I think, that there is no internal difference which we can detect between dreams and waking experiences, illusions and veridical perceptions, that enables us to distinguish them. The argument: Parents may have a female child, A and B are parents, therefore A and B may have a female child—is valid enough. "May" is to be understood relatively to the evidence available, and I think that the argument leading to the conclusion that I may be female sounds absurd only because I already know that I am not female. If what is stated in the second premiss, namely, that I am a human being were really all that was known of me, it would be perfectly legitimate to conclude that I "may be female". And what is meant by "may be" here is not, I think, either that it is logically possible, or that some people are female, but that relative to the evidence it is not certain that the person concerned is other than female. Interpreted in this way the major premiss of the sceptic's argument about physical objects becomes: From the fact that a man seems to himself to perceive a physical object it never follows with certainty that he is really perceiving that object, and the conclusion becomes—it is not certain that I am perceiving a chair. Dr. Moore would no doubt say that this begs the question; but there is a reason given for the major premiss, namely, that we also seem to perceive physical object in dreams, when we are not really perceiving them. Why does this seem to provide a ground for the sceptic? Not merely because some members of the class, ostensible perceptions, are not real perceptions of physical objects. If there were a clear criterion internal to the experience by which we could distinguish those members of the class which are not real perceptions of physical objects from those which are, the argument would lose at least a good deal of its point, but the trouble is that no such criterion is available. The argument may therefore be restated as follows:

XUM

There is no empirically observable difference in the experience itself between perceiving a physical thing in waking life and seeming to perceive a thing of that sort in dreams.

"There is a physical object of a certain kind" is empirical knowledge if it is knowledge at all, therefore there must be an empirically observable difference between an experience in which I know this and an experience in which I do not.

Therefore I cannot say that I now know that there is a physical chair here, when I cannot, faced with just the same experience, know it in dreams.

This argument seems to me formidable. No doubt empirical tests that fall outside my present experience could be applied in order to confirm that there was a chair here; but Dr. Moore's claim is that I can sometimes know the existence of chairs by just seeing them without applying further tests, and also I might have a dream in which I seemed to be applying these further tests as much as ever I do in waking life. It may, however, be objected that it is not certain that we ever do have dreams in which there occur experiences exactly the same as any we have in our waking life. It may be for anything we can show to the contrary that there is always a difference of some sort in the way a dream feels as compared to the way in which a waking experience of similar content feels. As far as they can remember this is undoubtedly the case with some people; but there are others, vivid dreamers, who could not be sure of this but would be rather inclined to deny it, saying that some dreams seemed just as real as waking life. However, no one can be confident enough in his memory of dreams and his ability to notice his feelings while dreaming for him to be entitled to say with any assurance whether the experience of seeming to see, *e.g.*, a chair in a dream is or is not the same in quality as is the experience of seeing it in waking life. It is therefore by no means sure that the first premiss of the argument is true, and so the conclusion is not proved. But at any rate the premiss *may* well be true, and if it is true it seems to me that the conclusion follows. No doubt it might be the case that some experiences of perception in waking life exactly resembled experiences in dreams but that the members of the former class which did so were not cases of knowledge. It would generally be admitted that there are cases of perception, *e.g.*, at a distance or in a mist, where we are not entitled to be sure that the kind of object we think we see is physically there at all, and it might be that the only experiences of perception in waking life which resembled experiences in dreams were of that kind. In that case

I do not think that Dr. Moore's position would be threatened, since he does not claim that all cases of ostensible perception are cases of knowledge. But if there are cases of apparent perception in dreams which exactly resemble internally the kind of experience I now have when I seem to perceive the chair, it is very difficult to see how I can possibly know in the strictest sense of "know" that I am perceiving a physical chair. It is agreed that the knowledge of physical objects is based on our experience in perceiving; but if so how could the same experience be a case of absolutely certain knowing on one occasion and a source of error on another? It might well still lead to true judgements on some occasions and to false on others; but since the same empirical criteria do not in that case always go with true physical object judgements, we could not say that we were entitled to be absolutely certain that our experience justified the judgements about physical objects. We are therefore confronted with an argument which, if its premisses are true, leads to the conclusion that we do not know any physical object propositions with absolute certainty. The second premiss is clearly true; but it is uncertain whether the first is or is not true. If it is true it follows that we do not know physical object propositions with absolute certainty; and since it is doubtful whether it is or is not true, it seems that we must conclude at least that we do not know that we know them with absolute certainty. Most people undoubtedly have dream experiences which bear a considerable resemblance to those of waking life, and the suggestion that dream experiences still really *always* feel somewhat different is not supported by experience but is a mere supposition. I should therefore conclude from the argument that it is at least very uncertain whether we ever know physical object propositions in the sense of "know" which connotes absolute and complete objective certainty. (That we are often subjectively certain is not disputed by anybody.) Some philosophers say that this is the only proper sense of "know"; and if so we shall then have to conclude that it is very uncertain whether we know any physical object propositions to be true at all.

The second argument of the sceptic which Dr. Moore considers is the argument, also suggested by Descartes, that we cannot have certain knowledge of physical objects because it is always possible that there may be a demon at work who is deceiving us. He replies that the statement that there may be a demon who is deceiving us in our perceptions may mean two things. It may mean, firstly, that this supposition is logically

possible; but that is not the point at issue. Dr. Moore admits that it is logically possible that there might not be a chair there when he seems to perceive one; but he holds that this is compatible with his knowing in the strictest sense that there really is a chair. Secondly, it may mean that the supposition is not formally incompatible with anything he knows; but this, he says, begs the question. For, if he does know that there is a chair, he does know something formally incompatible with the supposition that a demon is deceiving him in the matter.

In considering this reply the doubt that strikes me is as to how I could know in any sense which connotes absolute certainty that there is a chair here without *first* knowing that a demon is not deceiving me. If I know directly that there is a chair here this, I think, would be quite possible, and I then do not see any objection to Dr. Moore's reply; but, if the proposition that there is a chair here can only be justified by some process of induction, as Dr. Moore seems to think most likely from his treatment of the next sceptical objection, I do not see how such a conclusion could be established inductively with certainty without first eliminating the rival hypothesis that a demon is deceiving me. And there are unsolved difficulties in holding that I can know physical object propositions directly.

The third sceptical argument is to the effect that physical object propositions are all uncertain because they can only be justified by some sort of inductive argument and induction can never yield certainty. Dr. Moore replies that this argument involves four assumptions: (1) that I do not know the propositions in question directly; (2) that they are not logically entailed by anything which I know directly; (3) that if (1) and (2) are both true, physical object propositions can only be established inductively; (4) that inductive arguments can never give certainty. Dr. Moore asks whether it is as certain that all these four assumptions are true as it is that he knows there is a chair before him. He is convinced that it is not, and if so the argument must be rejected as sacrificing the more certain to the less certain. Of the four assumptions in question, which he admits are severally all very plausible, he is least disinclined to doubt the fourth; but he thinks any one of them much less certain than is the proposition that he knows that there is a chair.

Now Dr. Moore's whole argument here is founded on the conviction that we know physical object propositions in the *ordinary* sense of "know", not in any special philosophical sense of

" know ". Therefore, if the ordinary sense of " know " does not involve the complete and absolute certainty claimed, the basis of the argument goes. It is this proposition that I shall maintain. I shall not contend that I do not know that I am sitting in a chair ; but I shall claim that " know " as commonly used is to be analysed in a way which is compatible with admitting the force of the arguments that Dr. Moore has rejected. But the going is very treacherous here, and it is necessary to make very careful distinctions if we are not to fall into a trap. For that a person " knows " is a surprisingly complex proposition. I had better therefore rather risk going too slowly than going too quickly, and in order to make my position quite clear I had better start by raising certain points that are too elementary to be in themselves very interesting to the reader.

There is no doubt that the expression " A knows that S is P " cannot be correctly used unless two conditions are fulfilled. (1) A proposition about A's state of mind which can be expressed by saying " A is subjectively certain " must be true. (2) It must also be true that S really is P. If I am A and " know " is in the present tense, it is not indeed possible for me to separate (1) and (2). For to be subjectively certain that S is P involves affirming at least to myself that S is P, so that I cannot affirm (1) without also affirming (2). This, I think, is the explanation why the two propositions are both expressed by the same sentence " I know ". But I might distinguish the two at some later time and say " I was certain that S is P but S really is not P ". In that case I should certainly not say that I ever knew that S is P but only that I thought I knew. Similarly, I can make the distinction in regard to other people and say that they are certain of something which I do not think true, but I shall not then (unless I am speaking ironically) say they know, but only that they think they know. I shall later make a distinction between a more popular and a stricter sense of " know ", but what I have said so far applies to both senses. In both senses it is the case that " A knows that S is P " entails " S is P ", whereas " A believes that S is P " or " A is certain that S is P " does not entail " S is P ".

The entailment in question is explained by philosophers in two ways. Firstly, it may be held that the proposition " A cannot know that S is P unless S is P " is only an analytic, verbal one following from the fact that " S is P " is part of the meaning of " A knows that S is P ". On this view knowledge is always true for the same reason as that for which treason never prospers, *i.e.* only because we do not call it knowledge if we do not think

it true. But, secondly, some philosophers, *e.g.* Cook Wilson, have held that there is a certain state of mind, knowing, such that it is absolutely impossible for a person who is in this state of mind to be mistaken as to the truth of the proposition towards which his act of knowing is directed. In that case the entailment would presumably be synthetic *a priori*, *i.e.* a certain fact about one's mental state in regard to the proposition that *S* was *P* would entail that *S* was *P*. I do not wish to discuss here the issue between the two rival views, but I am inclined to prefer the former. If it is adopted, "A knows that *S* is *P*" will be a highly complex proposition including both a proposition about A's state of mind and the proposition that *S* is *P* itself besides other constituents which I shall next mention. Such different propositions came to be conjoined and expressed by the same form of words, I think, because when the first person singular of the present tense is involved they cannot, as we have seen, be separated.

But I have not yet mentioned all the conditions which must be fulfilled if we are to say correctly "A knows that *S* is *P*". Suppose *S* was *P* and A was certain that *S* was *P* but he was certain of it because of a fallacious argument. He could not then be correctly said to know that *S* is *P*. I should not "know" that the square of 11 was 121, however certain I might be about it, if I only thought it to be true because I believed that the square of 11 was greater by two than the square of 9 and that the square of 9 was 119. Or, again, suppose *S* was *P* and A was certain that *S* was *P* but had no *reason* at all (as opposed to a psychological "cause") for his certainty. In that case he would still be said to *know*, if "*S* is *P*" were held to be a self-evident proposition or a proposition which could be established by his immediate perception, as could, *e.g.*, the proposition that he is or is not feeling cold, but not if it were held to be a proposition of a kind which could not be legitimately asserted except on the strength of reasons. Again, he would not be said to know it if he was certain of it for reasons which really only made it probable. We may express all these conditions by saying that we can only speak of "knowing" that *S* is *P* where "*S* is *P*" is *objectively*, as well as subjectively, certain. I do not put forward this account as a *definition* of "know" but as a rough explanation sufficient to make clear the use of the term. There are grave technical difficulties about giving a definition of "know".

Now philosophers have usually insisted that we must have absolute and complete certainty if we are to speak of "knowing"

at all; but this is not in accord with the most common usage of "know". It would certainly be according to any standard an incorrect use of language to employ the term "know" even of another person where the proposition said to be known is not thought by the person who uses the term to be true, or where though it is probably true its probability falls *much* short of certainty; but we do often speak of "knowing" where it would be admitted that certainty was not absolute. Thus we should use the term "scientific knowledge" without hesitation, although all or almost all the propositions of natural science covered by this term are not known with certainty but are very probable hypotheses or empirical generalizations. Again, suppose I hear on the wireless an official statement that some person of importance in Britain has died and am asked later whether I know that he has died. Almost everybody would say they did "know" under these circumstances, yet it clearly cannot be absolutely and completely certain that a single official statement might not be mistaken. We have thus a distinction between a stricter sense of "know" which philosophers have usually employed and a looser sense in which the word is actually used in everyday speech. If "know" is commonly actually used in the second way, I do not see how philosophers can have any right to say that theirs is the only proper sense of "know". This is a question of language, and in language the standard of propriety is the general usage of the educated. We may distinguish the two senses by saying that in the stricter sense of "know" the term is admissible only if we have complete subjective and objective certainty, while to use the term in the looser sense it is sufficient that there should be "practical" objective certainty and absence of real doubt. We commonly say we "know" that S is P where we are practically certain, even if there is a theoretical possibility of error, provided it is very slight. If later on it does turn out to be the case that S is not P, or even if I discover reasons which leave room for a substantial risk that S is not P, I shall then deny that I ever knew that S was P; but as long as such evidence against it is not forthcoming, and what evidence there is makes it practically certain, we are content to speak of "knowing". No doubt whenever we do this we run a slight risk that our statement will not be true, for if it turns out that S is not P after all we shall, even according to the less strict sense of "know", have to deny that we knew; but then we do commonly make statements in regard to which there is a *slight* risk that they will not turn out true, and this does not mean that the statement "I know"

XUM

will be false because there is a slight risk that I am wrong. If the risk materializes it will be false, but the odds are *ex hypothesi* very much against that happening, and if it does not materialize and S is P the statement will be true. If we were using "know" in the strict sense there would be not merely a risk but a certainty of error every time we used it in the absence of the completest certainty, objective and subjective, that S is P.

It is a very common circumstance for a person, when he says he knows something, to be only vaguely and not explicitly aware of the reasons for accepting its truth. Most of the people who said they "knew" that so-and-so had died because this had been announced on the wireless would not be distinctly aware, at the time at least, of the reasons which justified a more confident acceptance of this information than of a statement on the wireless by some politician that, *e.g.*, imperial preference is a good thing. And certainly most people when they claim knowledge of particular physical object propositions are not distinctly aware of the circumstances which justify them in passing beyond their own sense-data. Yet if "know" is to be used in the stricter sense, I think it can hardly be applied with consistency to anybody who is not quite clearly aware of the reasons which justify him in asserting the truth of the proposition of which he claims knowledge. In view of this and of the philosophical difficulties raised it seems to me very difficult to maintain that in the stricter sense of "know" we know any physical object propositions at all. It also seems ridiculous to me, as it does to Dr. Moore, to say that I do not know whether this is a table or whether I am standing or sitting; but this, I think, is because we are here using "know" in a second and less exigent sense, which is also the sense in which most people in everyday life use the term. There are still philosophical difficulties which I have not been able completely to solve about holding that we know any physical object propositions even in the looser sense of "know", but despite the difficulties I cannot help being convinced that I do know them. Any argument to the contrary, however plausible, is at the worst less certain than that I thus know them. I wish at any rate to make it clear that any dispute between Dr. Moore and me is not as to whether we know physical object propositions but as to the analysis of the term "know". Similarly, I should say that it would convey quite a wrong impression to say that all physical object propositions are only "probable". We commonly use the term, "probable", not for what falls very slightly short of certainty but for what falls a great deal short, and therefore

for me to say that it is only probable that there is a table before me would be to express a much lower degree of certainty than I feel. For the same reason it would be wrong of me to say in ordinary conversation even that it was not certain; it is practically certain. But, if I am talking philosophy, this is not wrong, for it falls short of absolute hundred per cent. certainty. Dr. Moore's whole argument depends on the assumptions that we know physical object propositions and that "knowing" connotes absolute certainty; the former assumption I accept but the latter I reject, and if this course is adopted it becomes unnecessary to find arguments to justify their absolute certainty against the sceptic.

Philosophers who take the synthetic *a priori* view of the connection between the mental state of knowing and the truth of the proposition known are very inclined to say that there is only one correct sense of "knowing" and that the usage I have suggested is just wrong, but if it is an ordinary usage it is difficult to see on what grounds it could be condemned as wrong. But no doubt it would follow from their view that in cases where we "know" only in the looser sense our state of mind, and not only the objective relation of the proposition claimed to be known to the evidence, is different from what it is in the cases where we "know" in the stronger sense. For if it were the same in the first case as it is in the second, we should have objective certainty. But this should be, even for them, no reason for rejecting the looser sense: obviously our mental state of practical certainty, if not identical with, at any rate bears some considerable resemblance to the mental state of actual certainty, otherwise the two would not be, as they are, liable to be confused with each other, and this would explain why the term "know" was used in both senses.

It may even be contended that the notion of absolute certainty has no meaning as applied to physical object propositions, provided it is thought that these can only be established by induction and are not directly known. For it seems to be involved in the meaning of induction in the sense in which the word is used here that it cannot give complete certainty, since further tests which would increase the degree of certainty are always possible. If not analytically included in the meaning of "induction" it is a synthetic *a priori* proposition involved in its meaning in the sense of following necessarily from it. It may therefore be said that it was not appropriate to express what I had to say by saying that physical object propositions are not absolutely certain, since many of them are certain in the only sense in

which certainty can be applied to what is established by induction, so that to deny certainty of them is like denying goodness of food because food cannot be good in the sense in which a moral man is good. This criticism would clearly be justified if I was using "absolute certainty" in the sense in which this is applicable to *a priori* propositions; but that is not the only sense of absolute certainty. "Absolute certainty", in a somewhat different sense, also applies to directly cognised empirical propositions about one's present mental state and immediate objects of sense; and since it is logically possible that physical object propositions could be known directly, the question whether they are absolutely certain in this sense is a quite legitimate one. In fact the question whether physical object propositions were ever absolutely certain would resolve itself for me into the question whether they could be directly known in the strict sense of "know". My answer, which I have not time to discuss here, would be that they are directly cognised but not in such a way as to give absolutely certain knowledge.

This answer is given from a realist point of view; but I think it very important to realize that the acceptance of a phenomenalist analysis of physical object propositions would not make it any easier to hold that they were ever known directly but would rather have the reverse effect. If physical object propositions are analysed phenomenalistically they are analysed in terms not only of my present experience or sense-data but in terms of the sense-data of other persons and of my future possible sense-data. Now, while it may be claimed without absurdity, though perhaps wrongly, that I am directly aware of an independent physical object when I see the table, it can hardly be claimed that when I see it I am immediately aware of the sense-data of the other people in the room, still less (when I am alone) of the hypothetical non-existent sense-data of the "normal observers" who would see it under certain conditions or of the sense-data that I shall or should have under hypothetical conditions not yet realized. This, though not logically self-contradictory, is quite obviously false. The point I have just raised seems to me very important because the chief motive for adopting the phenomenalist analysis of propositions about physical objects seems to have been that it would enable one to claim knowledge of physical objects. But in fact, if "knowledge" is used in the strict sense, it is even more difficult to claim this than it is on the realist view, for while, if you are a realist, it is just possible, though difficult, to claim that we can have

direct knowledge of physical objects it is quite impossible on the phenomenalist view.

If we take the view that all physical object propositions are based on induction, it follows logically, I think, that they can never be absolutely certain in the sense in which directly known propositions about one's present state can be, though it remains true that there is a sense of "certain" in which they can very well be certain, *i.e.* the sense which I have expressed by the phrase, "practically certain". It may therefore be argued that it is meaningless to deny that they are absolutely certain, since absolute certainty is just a predicate that has no sense in this connection at all. But this does not follow. For:

1. The denial of a logically self-contradictory proposition is not meaningless. If it would be meaningless, which I do not admit, to say that some physical object propositions were absolutely certain in the sense in which propositions about my present state can be, the statement that they cannot be absolutely certain in this sense would become the statement that it was meaningless to say they were, and a statement that something else is meaningless is not without meaning itself. It has a meaning even to say that "abracadabra" means nothing, and since it has been at any rate supposed that the notion of absolute certainty could be applied to physical object propositions, while as far as I know it has never been supposed even by philosophers that "abracadabra" meant anything, a proposition denying meaning to the one would have a utility which the denial of meaning to the other would not have.

2. The proposition denied is not logically self-contradictory, since it is logically possible that physical object propositions might be directly cognized with certainty.

3. There is in any case a perfectly good meaning in saying that all physical object propositions are uncertain, whether there would be a meaning in saying that some are absolutely certain or not. For it is clear that, if physical object propositions can only be justified inductively, however many tests we apply, their degree of certainty may always be increased still further by applying others, and this entails a standard of certainty which is never completely reached. It may be held either that this standard is derived from other branches of knowledge, or that it is an unrealized idea formed as a result of the observation that one physical object proposition is more certain than another, as the idea of perfect goodness may be formed even in the absence of any experience of its realization by noting that one man is better than another and that, however good any man we en-

XUM

counter is, there is always a better possible. The same consideration prevents us saying that some physical object propositions are at least as certain as they could conceivably be, it being assumed that they could not conceivably be certain in any sense of "certain" other than that in which inductively established propositions can be certain, *i.e.* practically certain. For this practical certainty is not a fixed quantity which cannot be exceeded but a matter of degree. However certain the proposition that there is a table in front of me is in view of the evidence at my disposal, its certainty could be increased by additional tests. This sounds very odd, but I think only for the same sort of reason as it sounds odd to talk about the heat of something which is very little above absolute zero. Just as outside the study of physical science we are not practically interested in very small degrees of heat, and describe things which possess very little heat as not hot at all, so outside philosophy when the uncertainty falls below a certain limit we take no notice of it and describe the proposition as certain. But that the certainty cannot be complete I think follows from the nature of induction, if the physical object propositions are based on this. We can in that case see that the premisses just do not entail the absolute certainty of the conclusion.

Therefore, if we have absolutely certain knowledge of physical objects, it must be direct. That we should have such knowledge is logically possible, but does it in fact occur? The view that we have such knowledge is defended by Professor Aaron in a recent number of this periodical.[1] If it is claimed that the knowledge is direct the last two of the objections to its absolute certainty which Dr. Moore and I have discussed disappear, *i.e.* the objection from the possibility of a deceitful demon and the objection from the nature of induction. There remains the objection from dreams, which seems to me strong, and that from illusions, which makes it exceedingly difficult to say what it is we know with absolute certainty. I know that there is a table before me. But the colour of the table is generally held to be a subjective appearance and would probably appear quite different if I looked through a microscope, its shape and size vary with my position, its texture would again appear quite different if I could see the ultimate particles. What is there left that is absolutely certain? The occurrence of illusion is not incompatible with the view that we have a direct cognition of physical objects which gives us an element of truth mixed with the

[1] Vol. LI, No. 204, pp. 311 ff.

illusion, but is it compatible with the occurrence of a direct knowledge that is absolutely certain? In the absence of correction by experience and instruction we are just as convinced of the physical existence of qualities like the colour of the sky or of mirror-images to which we later do not assign physical existence as of the qualities which we say really belong to the physical object. I am only justified in being even practically certain that there is a table here because such experiences have not deceived me in the past but have been confirmed by coherence with others.

II.—PSYCHOANALYSIS AND EXPERIMENTAL PSYCHOLOGY: A REVIEW FROM THE STANDPOINT OF SCIENTIFIC EMPIRICISM.

By Gustav Bergmann.

This paper proposes to discuss from a purely methodological angle, with occasional references to the history of ideas, the relationship between psychoanalysis and what is usually called experimental or academic psychology. 'Methodology' is here understood in that very general sense which is almost synonymous with the current expression 'philosophy of science'. Accordingly, the writer is neither an experimentalist nor a psychoanalyst, but a philosopher interested in the foundations of behaviour science; his epistemological frame of reference is that of Logical Positivism (Scientific Empiricism). The discussion, therefore, moves on a level of generality which, it is hoped, will justify its appearance in a philosophical magazine. But it must also be said that if terms like 'mechanistic' and 'deterministic' have been used with the clear-cut meaning that they have in science, this should not be interpreted as a rejection of their more subtle epistemological analysis which the empiricist *qua* philosopher is also bound to perform. The point is that, from the standpoint of Scientific Empiricism, those finer distinctions are almost completely irrelevant to the methodological schema of contemporary behaviour science. If they are urged upon the scientist, this is usually done in order to make him subservient to some metaphysical residue.

I.

Psychoanalysis was and still is, for better or worse, not only a scientific approach, but also a cultural phenomenon of broader scope and rather deep impact. In this respect it is comparable to Marxism and Darwinism. These, too, in their times transcended the limits of their respective fields, and, partly as symptoms, partly as causal factors, played a very conspicuous rôle in the formation and transformation of our ideological structure. Small wonder, therefore, that men of letters and philosophers paid so much attention to psychoanalysis. From the scientist's viewpoint this attention has not always been very helpful; it has rather added to the confusion by emotionalizing and reading immediate cultural significance into scientific issues

XUM

which, if at all, are only of indirect cultural impact. Let it be added, by way of clarification, that the kind of philosophy which most eagerly rallies around or chooses to combat such movements has little to do with that very theoretical, entirely analytical, and entirely non-evaluative criticism of the sciences and of knowledge in general in which modern empiricism sees its main task. Thus I shall say very little about those broader implications of psychoanalysis or, to use Freud's own and very appropriate term, of the psychoanalytic movement; nor shall I say anything about the merits of psychoanalysis as a therapeutic method, but, instead, proceed to characterize some of its more fundamental theoretical aspects.

Within the broad and, as far as precise knowledge goes, still rather loose framework of the history of ideas, the rise and structure of psychoanalysis seems to have been contingent upon and continuous with three main factors. These three factors, very different though not causally independent from each other, are: *first*, the group culture out of which it grew, *second*, those elements in the nineteenth-century thought-pattern which are frequently referred to as Darwinism, and, *third*, and finally, certain trends in German metaphysical and literary philosophy which are best epitomized by such names as Schopenhauer and Nietzsche. Since the Viennese background is of merely anecdotal interest, I shall not say more about it.

Freud himself, in his autobiography, has acknowledged the influence of Darwinism upon his thought. What I mean by Darwinism or, as I shall also call it, functionalism, is the repeated drawing upon the functional or adjustment aspect of acquired responses. The term 'functionalism' is therefore used here in the specific sense in which it applies to a later American school of psychology. This group of psychologists, probably also under the cultural impact of Darwinism, mistook adjustment value for an explanatory concept, whatever this latter term 'explanatory concept' might mean. For it is never the concepts which are explanatory, but only the laws and theories which are formulated in terms of these concepts. By laws I mean nothing but the regularities or correlations arrived at and generalized from observation, and by theory, roughly speaking, a logically integrated body of such laws. What the word 'logical' here refers to looks reassuringly like a textbook of mathematics. This again by way of clarification and in contradistinction to certain types of traditional philosophy.

The Darwinistic background is, I believe, responsible for one of the greatest virtues as well as for one of the most serious

limitations of psychoanalysis. The virtue I see in the hypothesis that the prime motivators, such as hunger, thirst, pain, avoidance, and sex, are the driving power, the source of energy behind the whole infinitely complex pattern of socio-psychological behaviour. It is only fair to say that psychoanalysis has pioneered by advancing this hypothesis on culturally tabooed grounds, but it is also true that this basic naturalistic assumption, as one might also call it, is nowadays not only common ground but commonplace for all behaviour sciences. And, what is more important, to-day it is also realized that expressions such as 'driving power' or 'source of energy', which have just been used, are, at best, loose and suggestive metaphors, not in themselves blue-prints of a scientific theory. That psychoanalysts are not always too clear on this point just goes to show that they, too, form no exception from the familiar sociological phenomenon that the erstwhile pioneers end up as stalwarts. I am here, of course, referring to the confusion which surrounds such terms as 'instinct', 'drive', and 'libido' in the psychoanalytical literature and, worst of all, to the completely wild and irresponsible speculations of Ferenczi's bioanalysis. So the objection on this score would be, not that psychoanalysts are too naturalistic, but rather that they sometimes do not do a good job of being naturalistic. On the other hand, I am deeply suspicious of the adverse criticism which comes from those nicifiers who object to psychoanalysis because of its being what they call a *drive psychology*. By this they mean that psychoanalysis bases its explanatory attempts upon the prime motivators; what they ignore, or want us to overlook, is that in this respect, psychoanalysis and contemporary experimental psychology do exactly the same thing. There is criticism and criticism. Writers like Miss Horney, as most clearly revealed by her criticism of the James-Lange theory on introspectionistic (!) grounds, are really the guardians of the good old spiritualistic tradition in philosophy, or, as psychoanalysts would phrase it, they are the carriers of the cultural resistance. I always feel like reminding those ladies and gentlemen that painting a picture is still painting a picture, in spite of the genetic connection which psychoanalysts claim to have discovered between these and other culturally less highly evaluated phenomena and activities. The values, after all, are not called into question if their occurrence is scientifically explained, and for the rest, one does not crusade for the place of variables in an explanatory schema. So there seems to be something wrong with the criticism from the right. The criticism from the left,

as one might call it, comes from experimental psychology and from those students of scientific empiricism who are interested in the methodology of the behaviour sciences. This type of criticism heartily endorses the naturalistic assumption of psychoanalysis; all it urges is to make the whole approach more and more concrete, less and less elusive and ambiguous; what it wants to promote is the integration of psychoanalysis, in a less sectarian and more democratic fashion, into the whole of our scientific knowledge. But to repeat again, the tough-minded naturalism which, historically speaking, psychoanalysis owes to its Darwinian background is, from this standpoint, one of its greatest virtues.

The disadvantage inherited from the Darwinian outlook I see in the propensity to teleological thinking and in the tendency to take teleological patterns for scientific explanation. To be more explicit on this crucial point: the observation that a certain type of response has survival value, or, more specifically as psychoanalysts express it, can be *understood* in the psychic economy of their patients, might help to focus a problem, but does not yet constitute a scientific explanation. Such explanations are to be sought in terms of the mechanisms by which the habit in question is acquired, not in terms of the so-called aims which it serves. To put the same thing differently in order to avoid misunderstanding: to be sure, the behaviour of organisms shows the most marked teleological or purposive features, but every scientific account of them in teleological terms is necessarily very superficial, really a description rather than an explanation, a subjective account rather than an objective prediction. The task or, at least, the ideal and methodological standard of any science remains always to derive those teleological features within the frame of a causal mechanism. Therefore, again, the stricture against psychoanalysis which could be made from the left is not that it is too mechanistic, but rather that, because of its immersion into functionalistic thought habits, it is not always mechanistic enough. The word 'mechanistic' is used in the very unphilosophical and very unemotional sense in which it is almost synonymous with 'scientific'. This time, too, I am rather confident that I express the general attitude of experimental psychology and again, I am afraid, I shall find myself in disagreement with writers like Miss Horney and the Gestalters.

Two further clarifications are best inserted at this point. The one is a criticism of psychoanalysis, the other a criticism of the nicifiers. Let us begin with the latter.

XUM

It is almost needless to say that all modern science is thoroughly deterministic. What remains after this high-sounding word has been deflated is, *first*, that a science which is not able to predict from an appropriate set of data the future course of all its variables has not yet solved its task, and, *second*, that there is no reason in heaven or on earth why this aim should be in principle unattainable to the behaviour sciences. Quite to the contrary, though we do not have too much to show in the behaviour field so far, still the whole frame of reference of both contemporary science and empiricism points strongly towards such a determinism, this in spite of certain much misunderstood developments in theoretical physics. Incidentally, one might add that whatever sense there is in the old metaphysical puzzle of the freedom of the will is completely safeguarded within the framework of such a scientific determinism, though the fear that such might not be the case provides one of the most powerful motives for all sorts of nicifiers, psychoanalytical or otherwise. What then is the meaning of the criticism that psychoanalysis is guilty of an entirely unjustifiable and destructive biological determinism? One must remember that only with this qualification as 'biological' has the objection any meaning, for without it the term 'deterministic', like 'mechanistic', means almost the same as 'scientific'. As far as I understand them, people who raise this kind of objection usually mean that psychoanalysis does not pay enough attention to the causal factors which are located in the organism's physical and social environment, and too much attention to allegedly hereditary and the so-called biological factors, which latter term is used for the prime motivators. Thus formulated the objection undoubtedly makes sense, and I am in no position to evaluate it. Only, it appears that this is not a methodological question about, but a pragmatic question within science itself as to the actual success of hypotheses which attribute, loosely speaking, different weights to the various sets of variables. As to the actual position of the psychoanalysts on this question, I am under the impression that our knowledge about the rôle of hereditary factors in personality development is still very rudimentary and that psychoanalysts, on the whole, have been rather careful not to commit themselves in either direction. As far as the environmental factors are concerned, psychoanalysis has, through the importance which it attributes to the family history and to such culturally conditioned processes as sublimation, taken account of them on the very groundfloor of its structure. How inadequate this structure may be is a different question, but it certainly cannot

be said that psychoanalysis has failed to provide for these factors within its systematic structure. There is, however, another aspect of this criticism which deals with the historical character of psychoanalysis. To this crucial point I shall return later.

Let me now turn to the last in this string of general remarks collected under the heading of Darwinism. This, as I have indicated before, is a stricture and not a defence of psychoanalysis. A direct comparison with so-called evolutionary theory will prove useful. In the generation of our parents Darwinism was often descried as a bold, all too bold theory, not sufficiently substantiated by facts and uncritically embraced for ulterior purposes by the radicals. As it presents itself to the modern methodologist, the situation shows, ironically enough, just the opposite picture. One can hardly think of a scientific fact better and more impressively documented than the phylogenetic hierarchy, established as it is by the threefold evidence of embryology, comparative anatomy, and paleobiology. As to the facts, nothing or next to nothing is left to be desired. On the other hand, it seems that there is as yet hardly any theory at all. For the observation that the species best fitted to a certain environment will ultimately replace other less fortunate ones is either a pseudo-teleological truism or a tautology. This just illustrates the point which was made before concerning the superficiality of teleological accounts. The real problem is to discover the biological mechanisms or, as Freud would call them, chemisms which regulate the transformation of species. A well-integrated body of such empirical laws, together with the data concerning the physical environment, would yield a causal account of the actual course the development of life has taken. This and only this would deserve the name of a *theory* of evolution, and for such a theory, as we know, experimental biology as yet has hardly laid the groundwork.

The analogy to psychoanalysis is obvious. Psychoanalysis, too, has discovered or rather, against the resistance of cultural taboos, forced our attention towards an impressive array of indisputable facts, but it has, to my mind, achieved relatively little in the way of a satisfactory, verifiable theory. The facts I have in mind are those of so-called infantile sexuality, the dream symbolism, and the decisive causal rôle which sexual factors, in the broad analytic sense of the term, play in the etiology of the psychoneuroses, in the formation of the normal personality, and in such group phenomena as the arts and folkways. These are, in a loose way of speaking, facts which

XUM

nowadays baffle only the old ladies, the literati, and the Gesalters among the scientists. As far as theory is concerned, however, I find nothing except one rather sketchy law which could be formulated in this way: An individual's personality mechanisms, that is, certain not too well-defined general response patterns, have something to do with his experiences and behaviour in certain crucial periods and/or with respect to certain crucial situations. Far be it from me to belittle the achievement which is represented even by this hypothesis and whatever elaboration it has been given, for it is probably the only step which has so far been made beyond the mere correlation of traits in this very difficult field of personality. But on the other hand, it seems absurd to consider such an ingenious vision, almost entirely devoid of any articulate causal determination, as the ultimate achievement, to be scholastically elaborated, not as just one of the starting-points of a theory of personality to be developed.

I shall not take the time to say more than a few words about the last of the three background factors, those speculative trends for which Schopenhauer and Nietzsche were chosen as significant representatives. These influences are most clearly noticeable in writings such as Freud's "Beyond the Pleasure Principle" and the theorizing or, as I should prefer to call it, the speculations about the so-called death instinct. As far as I can see, there is hardly any empirical content in this type of psychoanalytical literature and it takes much broad-mindedness and goodwill even to interpret these utterances as confused suggestions for alternative formulations of whatever theory there is. Here, I believe, the psychoanalysts have become the victims of their own metaphors and what lives on in the empty verbal shell is the ghost of a certain type of German metaphysics. It is fair to say, though, that Freud in his autobiography disavows any such influences. But does not psychoanalysis itself insist that what a man or a group recognizes as the motive or goal of his behaviour does not necessarily coincide with what the behaviour scientist calls its cause? Here again, by the way, we make use of an insight which was genuinely revolutionary at the time of Freud and Pareto and which is just common sense and almost commonplace in contemporary behaviour science.

II.

After this general stage-setting, I shall devote the rest of the paper to the elaboration of a thesis which has already been indicated. This, approximately, is what it says: The basic ideas and the systematic structure of psychoanalysis and of

contemporary experimental psychology are not, as it sometimes appears, in conflict with each other. Quite to the contrary, it is more accurate to say that these two approaches are, in their basic features, almost identical and, in a certain sense, namely with respect to the phenomena they study, complementary. Obviously, the one and only field which psychoanalysis is interested in is that which, in more academic terms, would be called *personality*. Experimental psychology, on the other hand, at least that part of it which is theoretically most significant, can well be said to centre around the so-called laws of learning. What is thus rationalized into a pseudo-opposition turns out, as it so often happens, to be a mere difference of interest. In the case of psychoanalysis versus experimental psychology, this apparent opposition is re-enforced, not only by certain institutional factors, but also by the circumstance that the field of personality research is not yet, and possibly never will be, amenable to the exact laboratory methods in which the experimentalists very justifiably set their greatest pride. But I see by now that the last few sentences, in order not to be misunderstood, require a good deal of explaining. So let us start on a new string of explanations and qualifications.

The statement that the psychology of learning is theoretically the most significant experimental field must not be taken as a disparagement of the work done in physiological psychology and in perception psychology in particular. What it asserts is only that perception psychology, if divorced from physiological psychology, is hardly of any theoretical interest at all, and that the *reductive* research of physiological psychology is, as yet, of little help in the study of molar behaviour. 'Reduction', in this context, refers to an explanation of molar behaviour by physiology; 'molar', as here used, covers a range wide enough to comprehend both the behaviour of the white rat in the learning maze and the symptoms of the psychoneurotic. Presently we shall see that within the behaviour field which, as a whole, is molar from the reductionist's point of view, the two phenomena just mentioned, animal maze learning and personality formation, lie far enough apart and are probably so related that they again can be opposed to each other as molecular (or microscopic) and molar (or macroscopic) respectively. These remarks should also elucidate our usage of the expression 'behaviour science'. It excludes, quite arbitrarily and for merely practical reasons, physiology and all kinds of reductive research.

A similarly qualifying remark is needed to explain the

attribution of the personality field to psychoanalysis or, more correctly speaking, to clinical psychology of the psychiatric type. Again this involves no disparagement of the careful work, usually classified as experimental, which has been done in such fields as intelligence and the correlation of personality traits, normal or abnormal, with each other and with environmental variables. The point is rather, first, that *some* of this work, valuable as it is as a collection of data, as a statistical stabilization and often as a wholesome correction of our prescientific insights, does not hold the promise of theoretical significance; second, that *nowhere* in this field has the necessity of a comprehensive theory, of a causal mechanism, been as clearly recognized as in psychoanalysis. It becomes necessary here to make a qualification within a qualification to the effect that so far this relative excellence of psychoanalytic theorizing has expressed itself not so much in the level of achievement or in the stringency of verification, but only in the underlying assumptions and in the systematic conceptions of the analytic approach. As to actual, reliable achievement experimental psychology seems far more advanced. On the other hand, it is fair to recognize that the tremendous difficulty of the task, which psychoanalysis set out to accomplish, has at least something to do with this undeniable difference.

Even after these qualifications have been made, the repeated use of the expression 'significance' calls for justification, for the term is evaluative, at least in the sense that it involves an appraisal as to the future course of science. 'Significant' has been used synonymously with instrumental for the construction of a factually adequate, genuinely causal theory. Such significance must not be confused with immediate social or therapeutic significance, though on the other hand we have of course good reason to believe that every theoretical progress will ultimately redound into increased social and therapeutic control. Any discrepancy between clinical and theoretical psychology which is construed by means of the ambiguity inherent in the term 'significance' is only a rationalized and often emotionalized difference of emphasis and interest. Clearly the outcome of a maze learning experiment is completely devoid of any significance in the latter, non-theoretical sense. On the other hand, it is at least conceivable that carefully worked out relationships between intelligence scores and environmental factors, in spite of their overwhelming social import, are not very interesting to the theorist. To explain this: it is at least conceivable that, within the systematic framework of a fully developed science

XUM

of psychology, those intelligence correlations will find their place as rather remote and complex corollaries of basic laws which have first been abstracted from the just mentioned rat experiments, while, on the other hand, the intelligence correlations themselves will not have yielded any such " theoretically significant " suggestions. Whatever bias was contained in the earlier formulation can now at least be very succinctly formulated: what has just been called a conceivable course of science is the course that science is most likely to take. This bias is apparently shared by the majority of the experimentalists, and, again, they find themselves in substantial agreement with the empirical core of psychoanalysis. To put the whole issue into a nutshell: The central idea of the psychoanalytic theory can very well be stated as an application of the laws of learning to the field of personality. To the elucidations and qualifications which such an *aperçu* requires we must now turn our attention.

A quick glance at physics will yield interesting cues concerning the way in which both the psychology of learning and modern empiricism formulate the task of behaviour science. Physics has now in all its branches reached the level of achievement first exemplified by Newton's theory of planetary motion. In this case, if the relative positions, velocities, and masses of the member planets at any moment are known to the physicist, he can, by application of his laws, compute or, as we also say, predict the positions and velocities at any earlier or later time point; he controls the process. The extent to which contemporary behaviour science falls short of this ideal is best elucidated in the following manner: *First*, we do not possess as yet such *process laws* of a closed system comprehending the organism and its environment; we must concentrate upon the organism as a system which interacts with its environment. The ingoing ones of these effects, issuing from the objective physical environment, are called stimuli, the outgoing ones responses. *Second*, because of this lack of functional equations describing the temporal unfolding of a closed system, we must rely upon the more primitive analysis in terms of cause and effect: In order to obtain lawfulness, we must watch for the regularities which exhibit themselves in the succession of isolated temporal cross-sections of the behaviour stream. Any such cross-section could also be referred to as a *state of the organism*, and each such state is at least partially characterized by its stimulus and/or response aspects. The qualification ' at least partially ' is of the essence of the argument, for, if we restrict our attention to the stimulus and response aspects of, for

XUM

instance, two successive temporal cross-sections or states, attempts to find lawful connections will fail. To put the same thing positively: As is well known, most of the responses we are interested in depend not only upon the features of the objective environment to which they are made, but also upon the organism's training and its drive state. The neglect of these additional variables undoubtedly vitiates a certain kind of early psychological theory known as associationism. Again a word of warning is needed. The word 'associationism' is now also used to refer to the stimulus-response technique of defining the variables of the problem. Clearly such a *technique* is not a *theory*, but rather, as we have seen, the methodologically correct way of formulating what all experimentalists and clinicians, no matter whether or not they call themselves S—R theorists, do. Attempts to discredit this technique by an emotive use of the ambiguous term 'associationism' or, still more colourfully, 'mechanistic and elementaristic associationism' clearly stem from and appeal to anti-naturalistic residues. Again psychoanalysis and experimental psychology are on the same side of the fence. In itself, reference to the two variable groups just mentioned as the organism's knowledge, perceptual or otherwise, and as its desires, conscious or unconscious, is of course not objectionable; objectionable is only the methodological confusion which slips in under the cover of the introspectionistic terms. The main offenders in this respect are not, as their fanciful terminology might lead one to expect, the psychoanalysts, but, because of their grounding in intuitionistic metaphysics, the Gestalters. What, on the other hand, keeps the experimentalists and the clinicians from committing the anthropomorphic fallacy is not so much an explicit epistemology, but rather their respective Watsonian and Darwinian backgrounds. The starting-point of the confusion is this: Different organisms, or, for that matter, one and the same organism at different points in his life-history, respond differently to one and the same physical environment. Hence, so the argument runs, the psychological environments, not to be confused with the physical ones, were different in each case and upon them the psychologist is therefore bound to concentrate his attention. Be it granted that the psychological environments were different; be it also granted that in the case of an articulate subject we could find out *some* times *some* things about his psychological environment if we could interview him before he acts. It will be noted that the point against which I am arguing has just been stretched to the utmost, for, strictly

speaking, the verbal report mentioned belongs to the overt behaviour to be predicted. But even so, what is the predictive value of the suggestive metaphor ' psychological environment ' ? Is it not the business of science to ascertain which objectivs factors in the past and present states of the organism and of ite environment account for the difference in response, so that we can actually predict it instead of attributing it, merely descriptively and after it has happened, to a difference in the psychological environment ? As in the case of teleological thinking, the animistic metaphor is mistaken for the answer to the question which it puts. It should not be necessary, though it might be wise, to stress at this point that the methodologically correct use of verbal reports, as data, and of introspectionistic terms, in the scientist's account of them, is not impugned. Even as much as to expect that they could be dispensed with in the field of human personality is mere utopia to-day, though a utopia whose methodological possibility can help us to clarify our ideas. One should realize that the denial of this possibility leads ultimately either to dualism or to Aristotelianism, possibly in one of its modernized emergentist forms. The particular problems connected with man's consciousness or, what for all scientific purposes amounts to the same, his symbolic responses, will be treated later. First, some remarks to complete this sketch of the experimentalist's schema are in order.

In the field of animal behaviour, where, for the obvious reason of relative simplicity, the greatest advances have been made in recent years, very successful definitions of the non-environmental variables have been achieved in such terms as food, water, sex deprivation periods, and number and temporal spacing of trials. Certain objective phases of the so-called consummatory or goal states, such as eating or the cessation of a nocuous stimulus, also play an important part. Re-enforcement theorists believe that these latter states are even indispensable if learning is to take place. This is not the place to formulate, by means of these *terms*, the most recent hypotheses as to the *laws* of learning, but two of the most important aspects of this body of interrelated hypotheses shall at least be mentioned. They are the well-known principles of conditioning and of generalization. The first states that under certain conditions, most important among which is probably temporal contiguity, a stimulus situation comes to arouse a part of a response not appropriate to itself but only to the succeeding goal situation. Pavlov's salivating dogs come here readily to mind. Significant is, furthermore,

XUM

the fact that, according to present conceptions, states of the organism such as these anticipatory salivations fulfil a steering and organizing function in that chaining of responses of which all learning consists. The principle of generalization states, roughly speaking, that of two objectively similar stimulus situations each will, to a degree depending upon the degree of their similarity, elicit the response originally acquired toward the other.

It is also noteworthy that the laws of learning are *historical* laws in the sense that at the present state of knowledge one cannot, as in the Newtonian case, predict the future course of events from any amount of data collected at the time point where the process to be predicted starts, but that one is forced to utilize additional information concerning the past life-history of the organism. It will help to throw light on this point as well as upon some earlier remarks if one points out that only with the actual achievement of the physiological reduction are historical laws likely to disappear from the theoretical core of the behaviour sciences. This also shows that the methodological distinction between historical and non-historical or systematic laws, important as it is, is not a distinction of principle in the sense that the occurrence of historical laws constitutes an *a-priori* characteristic of certain subject matters. But it seems undeniable that their essentially historical character is one of the most significant features psychoanalysis and present-day psychology of learning have in common. A last remark should further elucidate the point and forestall certain objections. If a rat enters the maze with a number of marks on his back which have been made there as a record of the numbers of trials through which he has been put, the *present* reading of these marks still amounts to the utilizing of historical information. Likewise, if the psychoanalytic patient now recalls and reports certain events of his life-history, he too, conveys historical information, though this fact is somewhat obscured by the additional circumstance that his recalling and telling it now is not so external to the ongoing process as the application and the reading of the colour marks in the case of the rat. All one has to do to justify this analysis is to point at the significance, both therapeutic and theoretical, which psychoanalysts attribute to the emergence of the actual historical truth during the course of a cure. In the last sentence the term 'historical' has been used in its customary connotation, not with the specific meaning in which it occurs in the rest of this paragraph.

XUM

III.

At this point of the discussion it should be obvious that I have not dodged my promise to talk about psychoanalysis and, under the sway of my behaviouristic prejudices, extolled the virtues of the white rat instead. As far as it is possible in such a brief survey, I even believe I have proved my point that the same basic ideas and the same frame of reference underlie both psychoanalysis and the experimental psychology of learning. In the phenomena of conditioning, for instance, and in the learning of more complex appetitive or escape (avoidance) behaviour with respect to beneficial or nocuous stimuli one recognizes the mechanisms to which psychoanalysts refer as the pleasure and the reality principle. Likewise, all those kinds of behaviour which, in a different terminology, are referred to as anxiety are based upon the general anticipatory responses to nocuous stimulations. Mechanisms like that of generalization contain at least the possibility of producing such personality mechanisms as displacement and projection. It will be noticed that the term 'mechanism' is here used in a rather vague and indiscriminate manner. The elimination of this ambiguity will be one of our last points. Let us first notice that already that halo of vagueness which surrounds such terms as 'drive', 'instinct', and 'libido' in the psychoanalytic literature has disappeared. What remains is this: Which consummatory states represent satisfiers or soothers for which hunger states belongs, within the limits of a non-physiological behaviour science, to the basic laws which must be experimentally ascertained. This is one of the three points at which psychology is supported by its so-called biological basis; all the rest is learning and therefore, though in a historical manner, environmental. Clearly, the term 'instinct' has no place in such a structure. If at all, experimentalists use it only to refer to certain very elementary unlearned stimulus-response connections like the lid reflex or the patellar response. These instincts or reflexes are the second point of contact where behaviour science, factually though not methodologically, rests upon its biological understructure. For, though this elementary outfit is decisive for the fate of the organism, it is evident that it constitutes the kind of material which psychology treats as data (basic laws). It provides the basis upon which psychology erects its explanatory structure. The explanation of this basis devolves, in turn, upon the biological sciences in the narrower sense of the term. The third point of contact is provided by

XUM

those individual constants, hereditary and/or constitutional, which make for individual differences not reducible to environmental factors. Incidentally, the last few remarks indicate, in a rough manner, what there is left of a hierarchical structure within the methodologically unified system of the sciences.

Lest the last paragraph be misunderstood as too sweeping an attack on the so-called libido theory, let me attempt a crude but plain formulation of what appears to be the empirical core of this part of the psychoanalytic theory. It will turn out to be the so-called theory of psychosexual personality development. The idea is that there are several pairs of hunger and goal states, corresponding to each other like thirst and drinking, which have the following properties: (1) the intensity of these hunger states varies, according to some biologically given maturation curves, during the organism's life span; (2) these hungers are so interrelated that, under certain conditions, goal states corresponding to one of them reduce the others (substitution); (3) all the hungers of this group and only these hungers, unlike sleep, food, water hunger, etc., are highly plastic in the sense that by some kind of generalization-like mechanism, they can be brought to accept secondary satisfiers (sublimation).

The possibility of sublimation clearly depends upon culture or, to say the same thing differently, upon the possibility of the symbolic response. By mentioning it we have therefore reached the stage where language and, with it, consciousness have to be taken into account. From this point on, little concrete information can be expected from the contemporary psychology of learning; at this juncture psychoanalysis takes over. This is, we recall, the respect in which psychoanalysis and the psychology of learning have been called complementary to each other. The situation is, however, not so simple as that, for in making this step we undoubtedly cross, in a certain sense, the borderline of reliable knowledge and shall have to be content with an as yet much lesser degree of articulateness and precision. Such a state of relative scientific ignorance is typically the one in which the philosophical methodologist can be of some modest use; for the philosopher with his prevailing interest in broad outlines will be naturally most eager to stress the discoverable structural identities and continuities. In the case at hand he will, of course, stress the stimulus-response view of language. In doing so he draws, historically speaking, upon the ingenious anticipatory vision first developed by G. H. Mead and that brand of social behaviourism that originates with him. Pragmatism, particularly as represented by Mead and Peirce, is,

XUM

on the other hand, also one of the historical roots of Scientific Empiricism. To observe such interrelations between the new philosophy and the new anthropology, in Kant's broad sense of the latter term, is certainly significant to the historian of ideas and, to the student of either field, very encouraging.

The verbal symbol, vocal or subvocal, and/or the mental image is, according to this view, a particular kind of intermediate response. That means that it originally occurs in a relatively long chain of stimuli and responses which begins, in presence of a primary hunger, with the external stimulus and ends with an overt response. Thus it lies, even in its most complex form, on a continuum with the anticipatory salivation of Pavlov's dogs. The difference in complexity, however, is no doubt tremendous and has many aspects. First, these symbolic states or symbols, as we shall shortly call them, seem to be particularly apt to establish horizontal connections between widely different stimulus-response chains. What this pictorial expression tries to convey is this: Imagine, vertically diagrammed, two different stimulus-response chains already established. Assume that the one leads ultimately to an overt appetitive act, the other to an overt avoidance response; assume, furthermore, that one and the same symbol or similar symbols (generalization!) occur in both chains. This is sometimes sufficient to extinguish or, as one also says, to inhibit the appetitive response. Under different conditions the appetitive chain will at least be modified by elements of the avoidance chain under consideration. The "many track" variability of such processes is further increased by the apparent ease of the mechanisms which establish associative connections of all kinds between the symbols. Second, to mention only one more feature, the external presentation of the symbolic stimulus alone, through speech or other symbolic devices, becomes very often sufficient to start in the organism one of those sequences of responses which follow it in one of the chains belonging to the habit pattern the individual has already acquired. The stimulus aspect of such an occurrence is just another complex case of learning; as to its motivational aspects, one can at least imagine that the cross connections just mentioned make it relatively easy to tap at least one of the just active hungers. Thus a new connection might become established and leave its trace upon the habit pattern or personality of the subject. At this point another feature should at least be mentioned which, according to Mead, operates in the communicative, culture-forming function of language. The war cry of the attacking animal "means" to him attack, while

XUM

to his victim it " means " flight. Less metaphorically expressed, the response chains called out in both organisms are different. A symbol, however, if occurring with and verbalized or otherwise expressed by a member of a cultural community, is apt to start what one might call, with considerable looseness, the same response chain in any of his fellows. The limits of this sameness coincide roughly with what in other contexts is referred to as the emotive limitations of communication.

Clearly, these few remarks are but a philosopher's map of scientific No Man's Land; even to consider them as the sketch of a learning theory of personality and of the higher processes would be inexcusable wishful thinking. But they can serve as an expository device and furnish the background for further elucidation of the relationship between the two approaches, psychoanalysis and the experimental psychology of learning. If there is any anticipatory truth at all in this speculative map, then the relationship in question is that between two corresponding molar and molecular levels, as these terms have been used in an earlier paragraph of this paper. It is convenient to begin with a clarification of the term 'mechanism'. So far it has been used ambiguously to denote, on the one hand, regular stimulus response sequences, both innate (reflexes) and acquired, and, on the other hand, the laws of learning which regulate their acquisition. This ambiguity is harmless; to point it out is sufficient to avoid confusion. More pertinent to our purpose is the fact that psychoanalysts and experimentalists if they both speak about mechanisms, speak about lawfulness on two different levels. Again an analogy from physics should help. At a certain stage in the kinetic theory of matter thermodynamic laws, like the formula of Boyle and Charles, are considered as consequences of the laws of Newtonian mechanics which operate, behind the surface, between the numerous particles of the substance. Certain statistical assumptions provide the very elaborate deductive connection between this micromechanism of Newtonian mechanics and the macromechanism of the gas laws. The illustration is meant to clarify the meaning of our terms. The point to be made can be stated in the following manner: The experimental psychology of learning studies the micromechanisms of which the so-called personality mechanisms of psychoanalysis are the very complex macro-result. The mediating theory, comparable to the statistical assumptions and their elaboration by Maxwell and Boltzmann, is so far missing. This decisive fact should not become obliterated by certain similarities between the respective mechanisms which

find their verbal expression in the use of such terms as 'inhibition' in both fields. Possibly these similarities are all very superficial; possibly, but not probably or improbably, for not even a hypothesis can be offered at the present stage of our knowledge. If one were in a position to do that or if this reduction were actually achieved, psychoanalysts, when asked for the explanation of a personality trait would not, instead of an answer, give that endless enumeration of possibilities which is so exasperating to the scientific mind. This shows that the methodological unification here attempted does not invite any rash optimism. Quite to the contrary, while it helps to eliminate pseudo-arguments, it also brings to the fore the difficulty of the task ahead. The main root of this difficulty lies, no doubt, in the tremendous complexity of the symbolic apparatus which mediates between man's prime motivators and his overt responses.

Let us, in conclusion, test the plausibility of this schema by inquiring what light it sheds upon the metaphorical terminology of the psychoanalysts. Within the limits of a broad, structural allocation one finds the *Id* rather satisfactorily represented by the prime motivators and those response habits which have been partially extinguished by the learning process. *Ego* and *Superego* are somewhat cruder conceptions. What they represent, personalized and endowed with some driving power of its own, is apparently the difference between the total pattern of the responses as they would occur before, and as they actually do occur after, the development of the symbolic apparatus during the learning process. *Conscious* are, by definition, those intermediate states of the organism which contain the actual occurrence of a verbal symbol and/or image. 'Intermediate' is always understood as referring to a response which stands, in an acquired chain, somewhere between the external stimulus and the overt response. The additional complication that vocalized verbal responses acquire culturally the value of overt non-verbal responses is here neglected. But even so it seems plausible that the symbolic responses are located relatively near the end of their chains and that the reaction is therefore less easily stopped short of overt expression if the process has once reached the conscious stage. Repression, that is the stopping of an initiated response sequence before it reaches that stage, becomes thus plausible as the result of association with strongly negative states; it also becomes plausible that anxiety, the general anticipatory response to such states, is aroused whenever strongly inhibited response sequences are about to reach consciousness. It is significant in this context and has been pointed

out by C. L. Hull that the first training of the libidinal hungers takes place in that period of life-history where the child's symbolic apparatus is still entirely missing or relatively rudimentary. So it seems indeed that we acquire much of our personality traits, like the rat its maze habits, through unconscious learning.

SELECTED REFERENCES.

Practically everything in this paper has been said clearly and explicitly, though in a more technical manner, by Professor C. L. Hull and his collaborators, N. E. Miller, O. H. Mowrer, R. R. Sears, and K. W. Spence. All the present writer can claim credit for is that twist of the argument which is due to his prevailingly systematic interest and his endeavour to promote mutual understanding between the behaviour sciences and the philosophy of Scientific Empiricism. Concerning the latter, non-technical information will be found in

1. Carnap, R., *The Unity of Science*, London, Kegan Paul, 1934.
2. —— *Philosophy and Logical Syntax*, London, Kegan Paul, 1935.
3. *International Encyclopedia of Unified Science* (Eds. Carnap, Morris, Neurath), The University of Chicago Press, Chicago, since 1938.

Compare, furthermore, the following writings by psychologists :—

4. Hull, C. L., " Modern Behaviorism and Psychoanalysis," *Trans. N.Y. Acad. Sci.*, 1939, **1**, Ser. II, 78-82.
5. —— " Memoranda Concerning Behavior Theory " (seminar reports on file in Yale University Library).
6. —— " Introduction to Behavior Theory " (Unpublished MS.).
7. Spence, K. W., " Theories of Learning " (chapter 12 in F. A. Moss, *Comparative Psychology*. Revised edition, New York, Prentice Hall, 1942.)

Concerning related problems of general methodology (philosophy of science) see also :—

8. Bergmann, G., and Spence, K. W., " Operationism and Theory in Psychology, *Psychol. Rev.*, 1941, **48**, 1-14.
9. Bergmann, G., " On Physicalistic Models of Nonphysical Terms," *Phil. Sci.*, 1940, **7**, 151-158.
10. —— " On Some Methodological Problems of Psychology," *Phil. Sci.*, 1940, **7**, 205-219.
11. —— " The Subject Matter of Psychology," *Phil. Sci.*, 1940, **7**, 415, 433.

The relation between those " disciplines " within or, rather, aspects of behaviour science which are usually referred to as psychology and sociology has been intentionally neglected in this paper. The societal and institutional analysis which characterizes sociology adds a new molar level with respect to which psychology as a whole is molecular. A sketch of this relationship within the frame of Scientific Empiricism can be found in

12. Zilsel, E., " Physics and the Problem of Historico-Sociological Laws," *Phil. Sci.*, 1941, **8**, 567-579.

III.—THE ALLEGED DEVELOPMENT OF BERKELEY'S PHILOSOPHY.[1]

By A. A. Luce.

Modern criticism is tending to disrupt and disintegrate the Berkeleian philosophy. I opposed that tendency in my article, "The Unity of the Berkeleian Philosophy", in Mind, XLVI, N.S., Nos. 181-182. There I dealt mainly with the external aspect of the case. I showed that Berkeley and his contemporaries were completely ignorant of his alleged change of views, and that his books fall under one 'design and connection', confirming and supporting one another. Finally, I gave a Doctrinal Analysis, which proves that the specific doctrines of the *Principles* are to be found in the later works. I have now to deal with the more internal aspect of the same case. I propose to state and examine the critics' doctrinal arguments, especially those based on the changes made by Berkeley in his later editions and on the supposed Platonism of the *Siris*.

The notion that Berkeley abandoned or modified parts of his early philosophy later in life originated, I think, in the Introductory Essays and the footnotes of A. C. Fraser's editions of the *Works*, especially the 1901 edition. Dr. G. A. Johnston took his cue from Fraser, and in his *The Development of Berkeley's Philosophy* (1923) he distinguished early, middle, and later periods of Berkeley's thought, and tried to trace a line of development from one to the other. Dr. Dawes Hicks in his *Berkeley* (1932) accepted most of Johnston's conclusions and widened their scope. R. Metz (*George Berkeley, Leben und Lehre*, 1925), Hone and Rossi (*Bishop Berkeley*, 1931), Dr. Kemp Smith (Mind, July 1933, p. 363), and Hedenius (*Sensationalism and Theology in Berkeley's Philosophy*, 1936) have endorsed in one way or another and furthered this opinion of disunity, which reached a climax when Professor J. Wild in his *George Berkeley* (1936) virtually denied that Berkeley ever had a settled system, and resolved the Berkeleian philosophy into a stream of phases.

The term *development* in this context is a euphemism; when critics say that Berkeley's philosophy developed, they mean that he discarded his early views and adopted new views; they mean that he changed his mind, and that his later books are at

[1] References to the *Three Dialogues* (abbr. *Hyl.*) give the page number in Fraser's edition (1901) of the *Works*, Vol. I; references to his other books give Berkeley's section number.

XUM

variance with his earlier books. I shall now state the case for this alleged development as it is presented by Fraser, by Johnston, and by Dawes Hicks.

Fraser speaks of the omission of the three sections on abstract ideas from the third edition of *Alciphron* as a withdrawal ;[1] he refers to the introduction of *notions* in the second edition of the *Principles* as a change of doctrine ;[2] he suggests that Berkeley replaced his ' early sensuous ideas ' by the Ideas of Plato ;[3] he speaks of his early nominalism as giving place to a later conceptualism;[4] and he finds in the *Siris* a new conception of external nature.[5]

Johnston works these points up into a systematic case, and follows it out into the epistemology, the ethics, and the mathematics, laying his foundations in a highly coloured account of Berkeley's *particularism, viz.* ' Almost every philosophical term which connotes a tendency to particularism may be predicated of the Berkeley of this early period. Sensationalist, atomist, empiricist, singularist, phenomenalist, solipsist, occasionalist—all these may be applied with greater or less truth to the writer of the *Commonplace Book*.' [6] He speaks of the alterations in the second edition of the *Principles* as a ' momentous admission ' ;[7] he makes the new point that Berkeley's letter to Johnson (1729) contains a disparagement of his early philosophy,[8] and he finds in the *Siris* a passage which ' reads like an apology for his early sensationalism '.[9] I should add that Dr. Johnston is at times fully aware of the other side of the picture, and he seems to me to leave nothing in his charge of sensationalism, and very little in his other charges when he writes, ' From the very first his architectonic conception has remained the same. The universe is an organic system dependent on God for its reality and its knowability. It is a spiritual unity, and the only forces that can work in it are spirits. This general *Weltanschauung* remains unchanged from first to last.[10]

Dawes Hicks begins by saying that Berkeley's theory emerged ' almost full-grown ', adding that in his later writings ' there are to be discerned not a few important modifications of his earlier doctrine '.[11] He appears to agree with Fraser on the omission of the sections on abstract ideas.[12] Of the two main

[1] *Works* (1901), I, pp. 218-219 ; III, p. 91 *n.* ; *cf.* III, pp. 8, 323 *n.*
[2] *Ib.*, I, pp. 219, 239 *n.*, 404 *n.* ; III, p. 272 *n.*
[3] *Ib.*, I, p. 239 *n.* ; III, pp. 126, 272 *n.*, 285 *n.*
[4] *Ib.*, II, pp. 341 *n.*, 344 *n.* ; III, p. 126.
[5] *Ib.*, III, pp. 119, 249 *n.*, 272 *n.*
[6] *Development*, p. 93.
[7] *Ib.*, p. 141.
[8] *Ib.*, p. 246.
[9] *Ib.*, p. 254.
[10] *Ib.*, p. 258.
[11] *Berkeley*, p. 25.
[12] *Ib.*, pp. 32-33.

alterations in the second edition of the *Principles* the one is, in his view, an admission tacitly conceding 'the whole point at issue',[1] the other 'an instance of letting in the waters that were destined to overflow the house'.[2] With Fraser and Johnston he assumes Berkeley's early nominalism,[3] but he goes beyond them when he comes to deal with 'the final phase', claiming to be able to detect development in the doctrine of immaterialism itself,[4] and speaking of 'that reconstruction of his speculative view which we find in *Siris*', and of 'many striking retractations of earlier doctrine'.[5]

The modern doctrine of development has here run riot. The critics' work, however well-intentioned, however off-set, as it is, by conservative statements, must inevitably discredit the Berkeleian philosophy. Our young students will not take Berkeley's contentions seriously if they are told that Berkeley himself withdrew them later in life. I do not for one moment believe that he withdrew them, and I shall now examine the foregoing charges *seriatim*. They look impressive *en masse;* under analysis they melt away; there may be a grain of truth in some of the charges if they are taken simply as vague statements about tendencies of thought and variations of emphasis; but there is no substance in any of them, in my opinion, if they are meant to prove that Berkeley, consciously or unconsciously, abandoned or modified the doctrines of his early philosophy.

Take first the sole piece of external evidence adduced, *viz.* Berkeley's letter to Johnson.[6] Johnston writes of it, 'In a letter written in 1729 he apologises for these early works on the ground that he was very young when he wrote them. "I do not therefore pretend that my books can teach truth. All I hope for is that they may be an occasion to inquisitive men to discover truth, by consulting their own minds, and looking into their own thoughts."'[7] Half-quotations may be misquotations. This is. Had Dr. Johnston noticed the preceding sentence, he would have seen there a remark which destroys his thesis and goes far to establish mine; for Berkeley wrote, 'What you have seen of mine was published when I was very young, and without doubt hath many defects. For though the notions should be true (*as I verily think they are*[8]) yet it is difficult to express them clearly and consistently. . . .' Far from expressing 'dissatisfaction with the bold conclusions of his youth', Berkeley looking back on them after twenty years categorically asserts that he

[1] *Berkeley*, p. 97. [2] *Ib.*, p. 162; *cf.* p. 146. [3] *Ib.*, p. 169. [4] *Ib.*, p. 206. [5] *Ib.*, pp. 208-209. [6] *Works*, II, p. 18. [7] *Development*, p. 246. [8] Italics mine.

XUM

believes them to be true. The modest words about defects of style and expression might be used by almost any writer when presenting his darling book to a friend. That truth is not taught but found in one's own heart was a commonplace of the Enlightenment. Note too that the whole quotation can be paralleled point for point (defects of expression, books as occasion for thinking out truth for oneself, and Berkeley's firm belief in the truth of his notions) in his statement about his *New Theory of Vision* in his *The Theory of Vision Vindicated* (sect. 33).

The Berkeley-Johnson correspondence[1] throws a flood of light on Berkeley's attitude in middle life towards his early philosophy. We see him asserting its truth, obviously convinced of its truth and zealously propagating it in the New World. He writes two long letters serially answering Johnson's objections, solving his difficulties and those of his friends. He invites Johnson to come and spend four or five days at Whitehall, his residence in Rhode Island, in philosophical conversation, and he sends him a consignment of his books. The correspondence occurred at the outset of his second great period of philosophical activity, which began with his enforced 'liberty and leisure in this distant retreat',[2] and lasted for six or seven years. During that period he repeatedly declared in word and act his strict adhesion to the philosophy of his youth; he defended it in his letters; he published new editions of the *Theory of Vision*, the *Principles*, and the *Three Dialogues;* he published the *Alciphron*, the *Vindication*, the *Analyst*, and the *Defence*—all of which defend or assert his early philosophy or at least build upon it and assume its truth. Throughout this long span of his mature life Berkeley had considered his theme from widely different angles; he had followed it up into theology and religion, into mathematics, and to some extent into psychology and physics. He had given his early philosophy a good trial, a long trial, and a trial over a wide field. It had stood the test; he was satisfied with it, and we see him there in his fiftieth year firm in his early philosophical faith. It is a reasonable presumption that his speculative opinions were fixed for life.

The opinion to the contrary, in so far as it is more than wishful thinking, is based on (1) Changes in the second edition of the *Principles*, (2) Changes in the third edition of the *Alciphron*, and (3) The contents of the *Siris*. We shall now consider these arguments *seriatim*.

[1] Johnson's letters are given in Jessop's edition of the *Principles* Appendix.

[2] Rhode Island; *Alciphron*, I, 1.

The changes in the second edition (1734) of the *Principles* are (*a*) To section 16 of the Introduction Berkeley adds three sentences acknowledging abstraction as the power of mentally omitting irrelevant details, (*b*) In sections 27, 89, 140, and 142 he adds sentences or phrases suggesting *notion* in place of *idea* to express our apprehension of spirits, acts, and relations, and in consequence he omits from sections 138-139 words which had refused the term *notion* in that context; from section 25 he omits 'notions', and in section 101 he adds 'and their relations'. All the other changes are doctrinally negligible.[1]

Take first (*a*). To section 16 of the Introduction Berkeley, in 1734, adds the words: 'And here it must be acknowledged that a man may consider a figure merely as triangular, without attending to the particular qualities of the angles, or relations of the sides. So far he may abstract; but this will never prove that he can frame an abstract general inconsistent idea of a triangle. In like manner we may consider *Peter* so far forth as man, or so far forth as animal, without framing the forementioned abstract idea, either of man or of animal, in as much as all that is perceived is not considered.'

Johnston and Dawes Hicks refer, respectively, to this addition as an admission and a concession. In point of fact it is neither. It is merely a repetition of the explicit teaching of the first edition. Berkeley always taught that there is a legitimate form of abstraction, which consists in considering separately, or 'singling' separable parts. He teaches it plainly in the Introduction, sections 10 ('To be plain, I own myself able to abstract in one sense . . .') and 15; he teaches it again in the body of the work, section 5 ('So far I will not deny I can abstract . . .'), and in section 126 he expounds it with great clearness and applies it to the universals about which geometry is conversant. There is a world of difference between abstracting by ignoring irrelevancies, and abstracting by combining inconsistencies; the former is a true process yielding true universals, the latter a false process yielding abstract general ideas on the model of Locke's ridiculous triangle, 'neither oblique nor rectangle, neither equilateral, equicrural, nor scalenon, but *all and none* of these at once'. I am not concerned here to defend Berkeley's doctrine; I am simply pointing out that what he says in the second edition about abstraction he had already said in the first edition. The paragraph in question adds emphasis, but nothing in substance new. By the way, Berkeley did just the same thing in 1710

[1] Jessop's edition of the *Principles* makes it easy to check these statements.

(the 1st edition); he already had the doctrine of *true* abstraction in his text quite clearly, but lest it should be overlooked he added in the Table of *Errata* that expression of it which now forms the latter part of section 10.

Turn now to (*b*). The additions and omissions about *notions* are all on the one point. It is a small, permissive change in terminology, without a shadow of change in doctrine. Berkeley teaches in both editions that we cannot have an *idea* of mind or spirit, because ideas are passive and mind and spirit active. He teaches in both editions that we have a true knowledge of mind or spirit, and he concedes in both editions that 'in a large sense', *i.e.* when we are speaking imprecisely, we may speak of an idea of spirit. All he does in the second edition is to *permit* (not *enjoin*) the technical use of the term *notion* in place of *idea in the large sense*, at the same time specifying acts of mind and 'relations including an act of the mind' as coming under this type of knowledge. Thus he preserves intact his fundamental distinction between active mind and passive idea, and obviates the damaging criticism, 'The Bishop of Cloyne denies that we can know God, or the mind, or acts of mind'. It is just a piece of practical wisdom; Berkeley says to his readers in effect, 'Of course we can know spirits and their acts; you can say we have an *idea* of them, if you like; but it is safer to say we have a *notion* of them'. He almost tells his readers not to make too much of this group of alterations; for he writes (sect. 142, 2nd edition) 'What I know, that I have some notion of. I will not say that the terms *idea* and *notion* may not be used convertibly, if the world will have it so. . . . But if in the modern way the word *idea* is extended to spirits, and relations and acts; this is after all an affair of verbal concern.'[1]

[1] T. E. Jessop in his Preface to his edition of the *Principles* has shown that the restricted use of the term *notion*, far from being 'unthought of when the *Principles* was written', as Johnston says (*Development*, p. 159), was considered by Berkeley in 1710—'the term only, not the doctrine it indicated, was omitted from the first edition'.

Berkeley was naturally reluctant to restrict the term and make it technical; for he uses it with great freedom in the *Principles* for an object of thought in general; *e.g.* 5, 9, 55, 56, 58, 66, 68, 71, 123, 141; also of abstractions and universals, 74, 81, 143, Introd. 14, 19. Berkeley never achieved precision in his use of the term. In *Alciphron* he uses it freely and vaguely, *e.g.* 'words, notions, and things' (IV, 12), 'notions of faith, opinion, and assent' (VII, 10), and it does not occur at all in his sustained account of knowledge in VII, 11-14. His most systematic treatment of notions is, curiously enough, in his *Discourse to Magistrates*, where he explains notions as prejudices. In the *Siris* the term is used both precisely (*e.g.* 'such are notions', 308) and loosely (*e.g.* 'notion of God', 288; *cf. Hyl.*, p. 448; 'notion of spirit . . . of body', 290).

But Johnston and Dawes Hicks magnify this 'affair of verbal concern' out of all proportion; they make of it a far-reaching change in epistemology; the new technical term, they say, signalises a tardy recognition of universals, and marks Berkeley's conversion from his early sensationalism, particularism, and nominalism to the conceptualism of his mature outlook.

Now the passages inserted in the second edition never mention universality or suggest it; Berkeley is not thinking of universal and particular, but of active and passive; he wants to avoid speaking of an idea of spirit, which, for him, is a contradiction in terms, like a round square (136), and he thinks that a notional knowledge of spirit, acts, and relations, will get round the difficulty. But notional knowledge here has nothing whatever to do with conceptual knowledge. It would be sheer nonsense to say, 'I have a conceptual knowledge of myself, or of Mr. Jones'; that is not what Berkeley intends when he says that he has a notion of himself or of other spirits. He simply means that he knows himself and knows other minds, but not in the same way as he knows things of sense. We must not reify *notion;* 'the proclivity of mankind to realise their notions' (*Siris*, 250) is, for Berkeley, the greatest and most dangerous idol of the tribe. Johnston and Dawes Hicks are here trying to 'realise' Berkeley's *notion*, which is neither a concept, nor a cognitive *tertium quid*, but simply a linguistic convenience denoting an active, spiritual object of apprehension, as distinct from a passive idea.

I have shown that the alterations in the second edition of the *Principles* have practically no doctrinal significance, and represent no *change* in doctrine whatsoever. But I cannot leave the matter there, and I must carry the war into the camp of my friends, the enemy. The critics have been misled about the second edition, partly because they have been misled about the first edition. They have taken over from their predecessors clichés like 'early sensationalism, particularism, and nominalism' which, attributed to Berkeley, are pure fabrications. Berkeley could never grow out of sensationalism, particularism, and nominalism, because he never was a sensationalist, a particularist, or a nominalist, as I understand those terms. He was interested in the senses, studied vision to some purpose, believed in the evidence of the senses and used it with devastating effect against materialism; but that does not make a man a sensationalist, unless we all, sensible sentients, are to be dubbed sensationalists. Sensationalism is the doctrine that all our knowledge is by the senses, and of the sensible. That doctrine Berkeley would reject out of hand in 1710, as later in life.

And what is particularism? I suppose it is the doctrine that particulars are everything, and universals nothing. That label is out of place here. Like a sensible man Berkeley was interested in particulars, and tried to read the general features of the universe in its particular constituents; but he nowhere denies or depreciates the universal and the general, and he writes that he fully agrees 'that all knowledge and demonstration are about universal notions'.[1] From the Draft Introduction[2] we see that he had deliberated upon this statement, and did not intend by it to subscribe to the Schoolmen's doctrines of universals; but quite early in life he had found a very reasonable theory, *viz.* that the universal is the significance of the particular. He sketches the theory in section 15 of the Introduction, and applies it in detail in section 126 to geometrical 'theorems and demonstrations'. Again, from his many references to set rules, established methods, connexion of ideas, laws of nature, and the constant regularity, order, and concatenation of natural things, it is obvious that he was well aware of the ground-work of universality in all particulars; and of course universality is the nerve of his doctrine of signs.[3] He was as anxious to preserve and stress the universal element in knowledge as he was to lay the bogey of the abstract general idea. There is no difference in the editions here, and therefore the term *particularist* applied to him neither in 1710 nor in 1734.

As for nominalism the author of the *Principles* was no nominalist, unless the author of the *Siris* was so too. Berkeley uses the term only once, I think, *viz.* in the Draft Introduction,[4] where he distinguishes his views on general names from 'the opinion of that sort of Schoolmen called Nominals'. There is no accepted definition of the term to-day. If *nominalist* merely mean *empiricist*, of course it applies to Berkeley, who was an apostle of the higher empiricism both in youth and age; but if it mean rather, as is suggested by the *Encyclopædia Britannica*, one who holds that universals are mere names, or one who takes an anti-spiritual view of the universe, it is inapplicable to Berkeley at any period of his life.

So then Berkeley's 'early sensationalism, particularism, and nominalism' are without basis in fact; they are inventions of critical ingenuity, framed and hammered together to serve as a *terminus a quo* for that mythical process—the development of Berkeley's philosophy.

We come now to the allegation that in his old age Berkeley

[1] *Princ.*, Introd., 15.
[2] *Works*, III, p. 368.
[3] *Princ.*, 30-31, 65; *Alc.*, VII, 11-12.
[4] *Works*, III, p. 366 *n.*

XUM

withdrew his attack on abstract general ideas. The charge is so baseless, and the case for it so flimsy, that I shall not waste much time on it, nor dignify it with an elaborate refutation here.[1] Dawes Hicks has tried to find support for the charge in the *Siris* (see below, pp. 150-151) which was published in 1744; but he has not succeeded; and the charge really rests entirely on Fraser's mistake about the third edition of the *Alciphron* (1752). If, as was undoubtedly the case, Berkeley held firmly to his early doctrine of abstraction in 1752, he could not have given it up in 1744.

In the third edition of the *Alciphron*, which appeared some six months before his death, Berkeley omitted from the seventh dialogue three entire sections on abstract ideas. Their titles in the original table of contents were:

5. Abstract ideas what and how made.
6. Abstract general ideas impossible.
7. In what sense there may be general ideas.

Berkeley does not tell us why he omitted the sections: he may well have left them out because their style is dry, dull, and academic, and the book, a popular defence of Christianity, reads better without them. However that may be, the argument that because he omitted the sections, he withdrew the doctrine they contain is childish. We might as well infer that he gave up his belief in the Resurrection, because he omitted from dialogue VI a long passage on that article of the Creed. Omissions, however striking, must not monopolise attention, but must be construed in the light of (*a*) what is left standing, and (*b*) insertions, if any. Fraser failed to consider (*a*) and (*b*). His eye was caught by the omission, and he did not look at the other side of the case. I do not think he could have read the third edition at all critically. Had he done so, he must have seen, (*a*) that it contains all the doctrine of the Introduction to the *Principles* on abstract ideas in undiminished force, and (*b*) new sentences inserted (VII, 12, 14) to point out that relations come under the doctrine of signs, and that 'these relations are not abstract general ideas'. Of the thirty-one sections in dialogue VII, fourteen sections contain references, direct or indirect, to the falsity of abstract general ideas. One quotation will suffice; for it is decisive. It is the climax of section 5 (3rd edition).

Alc. Will you not allow then that the mind can abstract?

Euph. I do not deny it may abstract in a certain sense:

[1] For a fuller discussion see my *Berkeley and Malebranche*, Ch. VIII.

inasmuch as those things that can really exist, or be really perceived asunder, may be conceived asunder, or abstracted one from the other; for instance, a man's head from his body, colour from motion, figure from weight. But it will not thence follow that the mind can frame abstract general ideas, which appear to be impossible.

Those words repeat the argument of the *Principles* (1710) and make it certain that Berkeley did not withdraw or weaken on his early doctrine of abstraction, but that he stuck to his guns to the end of his life.

The remainder of my paper deals with the *Siris*, the book of Berkeley's old age. Few read the work to-day, but many have been taught to think that it unsays a good deal of the teaching of the *Principles*. Here I shall take Dawes Hicks as the chief exponent of the critics' case; for he goes further than Fraser and Johnston, professing to find in the *Siris* 'reconstruction', and 'many striking retractations', including a virtual abandonment of immaterialism.[1]

The *Siris* is an unsystematic work with a variety of aims. In it Berkeley gossips and chats with his public. He wishes first to 'communicate to the public the salutary virtues of tar-water', then to break a lance with the mechanist philosophy, incidentally to foster the study of Greek philosophy, and finally to commend theism and trinitarianism to the reason of thinking men. He announces no new system; he announces no withdrawal of the old system. If the Bishop had meant the book to be a withdrawal or retractation, he was not frank with his public. I cannot believe that a man of his intellectual integrity and love of truth, to say nothing of his eminence in the world of letters, would thus fold his tent and 'silently steal away'. It is equally hard to believe that he changed his views without knowing it. The fact is that in the *Siris* Berkeley neither attacks his early philosophy, nor consciously defends it; it was part of himself; he assumes its truth, and quietly builds upon it. He was in his sixtieth year; the metaphysic he had pertinaciously defended for over thirty years had entered into his outlook, into the fabric of his mind, into his religion and into his ways of thought and speech. Analyse his vivid phrase for fire and light, 'the bright and lively signatures of a Divine Mind';[2] the writer who coined it was steeped, past cure, in sense-symbolism, immaterialism, and the vision of things in God.

Of the specific instances of retractation, adduced by Dawes

[1] *Berkeley*, pp. 208-209. [2] *Siris*, 173.

Hicks, the first is that 'In *Siris* . . . abstract ideas are called for and are, indeed, indispensable (§ 297)'.[1] The charge is not developed in any detail. The only reason that I can see for the reference to section 297 is that in it Berkeley makes a quite untechnical use of the term *abstract*, *viz.* 'notions, principles, and verities . . . abstract from sensible matters'. He is not accepting abstract ideas, but is mentioning instances of supersensible knowledge, which is accepted as freely in the *Principles* as in the *Siris*.

Absolute space, to which Dawes Hicks refers, is strongly condemned in the *Siris* (271), and so too are the abstractions of physics, elastic ether, gravity, force, and hamate atoms (231-249). Formal treatment of abstract ideas is not to be expected in this unsystematic work—'rude Essay' as it calls itself (297)—but I find in it two references, approving references, to the early doctrine of abstraction. In section 335 Berkeley distinguishes Plato's ideas from 'abstract ideas in the modern sense'; and in section 323 having to mention Plutarch's term for Deity, *χωριστὸν εἶδος*, he adds the comment, 'not an abstract idea compounded of inconsistencies, and prescinded from all real things, as some moderns understand abstraction'—where the allusion to Locke's 'absurd triangle' and his own attack on it is unmistakable. This item in the charge is unsupported by evidence, is refuted by the facts, and looks like Fraser's charge against the 1752 edition of *Alciphron*, answered above, pp. 149-150, read back into the *Siris*.

The next supposed retractation consists in 'a dislodgment of sense-perception' and a 'recognition of the essential function of conceptual thought'.[2] The answer to this vague charge is that Berkeley in youth and age honoured his senses and his reason (see above, pp. 147-148). In his first great book he studied vision and the visible by aid of reason; and there he is careful not to isolate vision from the judgement, and he points out that seeing is perceiving by sight. He never supposed that knowledge is acquired by standing in front of things with open eyes, like a cow contemplating Killarney. 'Sense supplies images to memory. These become subjects for fancy to work upon. Reason considers and judges of the imaginations. And these acts of reason become new objects to the understanding. In this scale, each lower faculty is a step that leads to one above it' (*Siris*, 303). This well-known passage is an epitome of *Alciphron*, VII, 13, and schematises the epistemology with which Berkeley works in all his books.

[1] *Berkeley*, p. 209. [2] *Ib.*, p. 210.

We now come to 'another important departure from empirical doctrine'.[1] Here Dawes Hicks raises the gravest issue; he as good as says that Berkeley when writing the *Siris* had given up immaterialism, had fallen back on the Platonic or neo-Platonic theory of ideas, and had accepted matter, second causes, and mechanical agents. If this were a true charge (and I hope to show it is mistaken) the *Siris* puts an end to all that the Berkeleian philosophy stands for.

Let us take causality first, though matter and the material cause cannot ultimately be separated. The sole ground for supposing that Bishop Berkeley went back on his early doctrine of cause is that he writes (*Siris*, 160), 'Without instrumental and second causes, there could be no regular course of nature'. Dawes Hicks quotes that sentence, but gives only a paraphrase, not fully adequate, of the rest of the section. When the section is read as a whole, it is clear that the second causes admitted are 'physical agents improperly so called . . . necessary to assist, not the governor, but the governed'. In other words, they are not efficient causes, not causes *a parte Dei*, but *effects*, which man for his own purposes treats as causes. The true name for them is *signs*, and in calling them *causes* occasionally, Berkeley is acting on his own precept, delivered about natural causes (*Princ.*, 51), 'to think with the learned, and speak with the vulgar'. He nowhere teaches efficient second causes other than the mind of man. The absolute passivity of all objects of sense is heavily stressed in the *Siris* (*e.g.* 154, 220-221,245-250, 257-258, 291-292) as in the other works; and therefore his second causes here are simply the signs, or significant effects, which in all his books constitute the course of nature under God, 'there being a certain order and connexion between them, like to that of cause and effect' (*Princ.*, 64). He says as much in the *Siris* (154), 'When, therefore, we speak of corporeal agents, or corporeal causes, this is to be understood in a different, subordinate, and improper sense'. Berkeley's doctrine of cause, therefore, remained unretracted, and unchanged.

Of matter Dawes Hicks speaks with caution; he is decidedly of opinion that Berkeley changed his views, but he leaves the precise direction of the change undetermined, offering us the choice between Plato's ideas and Aristotle's matter. He thinks that the *Siris* veers sometimes in the one direction, sometimes in the other. He says rightly that in the *Siris* the term *idea* in its old sense almost [2] disappears, and is replaced by *phenomenon* or *appearance*, and he argues (wrongly, in my opinion) that

[1] *Berkeley*, pp. 211-213.

[2] It occurs once, sect. 308.

Berkeley having called objects of sense *appearances* is bound to go further and ask, Of what are they appearances? Hence the dilemma—Plato or matter.

I reply, first, that the abandonment of the term *idea* is no proof of a change of doctrine. The right to change a technical term to suit the changing times must be conceded to every thinker. In our day what a number of technical terms have been in use and temporary vogue for the immediate object of sense, sensation, impression, phenomenon, presentation, *sensibile*, sense datum, *sensum*, etc. What writer could claim to have been consistent throughout one book,[1] let alone thirty years? The suggestion of such terms is for ever changing. *Idea* conveyed Berkeley's meaning in 1710, and no longer did so in 1744, Locke's usage having in the meantime prevailed.[2] Berkeley was never wedded to the term, and often apologised for it, and used alternatives in his early books. *Phenomenon* and *appearance* are no novelties in the *Siris;* they occur too in the early books. Here is a good instance from the *Three Dialogues* (p. 460)—

Phil: What mean you, Hylas, by the *phenomena?*

Hyl.: I mean the appearances which I perceive by my senses.

Phil: And the appearances perceived by sense, are they not ideas?

Hyl.: I have told you so a hundred times.

The important point is not the term, but what the term stood for. In 1710 Berkeley used *idea* to express, *inter alia*, immediacy and necessary relation to mind; in 1744 he uses *appearance* for the same purpose. 'All phenomena are, to speak truly, appearances in the soul or mind; and it hath never been explained, nor can it be explained, how external bodies, figures, and motions, should produce an appearance in the mind' (*Siris*, 251). There, just as in *Princ.*, 3, are the New Principle and the intuitive disproof of matter. Of course had Berkeley gone on, as Dawes Hicks presumes, to ask, Of what are they appearances? his immaterialism and the whole of his early philosophy might have gone down into the pit of representationism; but he never asked that question; he could not have done so. Ideas of sense, phenomena, appearances, etc., alike in 1744 and in 1710, were to him realities, and never copies. 'They are, therefore, such as we see and perceive them; their real and objective natures are, therefore, the same' (*Siris*, 292).

[1] In his *Critical Realism* Dawes Hicks sometimes uses *sensum* sometimes *sense datum*, not to mention *content* and *appearance*.

[2] 'Perhaps I rather restore the word, idea, to its original sense, from which Mr. Locke had perverted it.' Hume, *Treatise*, I, l. 1 *n*.

Berkeley does not formally deny matter in the *Siris;* but he speaks and thinks as an immaterialist; he looks for immaterialism, or the next best thing, in the Greek philosophers; in scores of sections he attacks materialism under the name of mechanism; and if he were not an immaterialist when he wrote the *Siris*, the greater part of that book is inexplicable.[1]

I conclude with a brief study of the alleged Platonism of the *Siris*. Plato influenced Berkeley, as he influences almost every thinker, and of course Berkeley approved much in Plato's teaching, as we all do; but that influence and that approval were by no means confined to the *Siris* period. Berkeley read Plato in his student days; as early as 1709 we find him recommending the *Phædo* and *Crito* to Sir John Percival;[2] from his earliest days of authorship he was steeped in Malebranche, and was therefore at home in 'the Platonic tradition'. In his essays in the *Guardian* (1713) he commends Plato, quotes the *Gorgias* and *Theætetus*, and refers to 'numberless other passages of his writings';[3] the *Alciphron* too contains many references to Plato. The view that Berkeley came to Plato late in life and was thus induced to recast his philosophy is baseless.

References to Plato and Platonism are scattered at random throughout the *Siris*, and I have collected the passages and arranged them roughly under topics, as follows:

PASSAGES ON PLATO AND PLATONISM IN THE *SIRIS*

Plato on hylozoism, 153, 276; on the fire philosophy, 166, 210-212, 281-283; on air, 168; on wine, on tar, 212; on the chain of being, 274-279, 284; on fate, 271; on matter, 306, 317-318; on absolute existence, 311; on the existence of sensible qualities in the mind, 316; on human intellect and soul, 171, 178; on the lapsed state of the human mind, 302; on the divine in man, 345; on the unity of self, 346-347; on cause, 260; on knowledge, 253-254, 304-306; on reminiscence, 313; on innate ideas, 308-309; on ideas, 335-338; on God, 320, 322, 328-329, 342, 344; on God and the soul, 268, 334; on God and the world, 300, 343; on the Trinity, 352, 358, 365.

[1] Note the condemnation in sect. 271 of 'external existence'—virtually a synonym of matter, and the association with it of absolute space and motion; also in sect. 319 the argument that gravity is proportional to matter is a 'vain circle', exactly as in the *Commonplace Book*, 627, and *Princ.*, 103.

[2] Rand, *Berkeley and Percival*, pp. 65-69.

[3] *Works*, IV, pp. 145, 146, 171.

Plato, not an atheist, 354 ; fosters study of the supersensible, 366-367 ; and the older tradition, 298, 339.

Praise of Plato, 332, 350 ; summary of the Pythagoreans' and Platonists' notion of the true system of the world, 266-267.

A glance at this analysis shows that Berkeley's interest in Plato was wide and deep. He habitually brackets the Pythagoreans with the Platonists, and often uses the latter term where we should use neo-Platonist ; but without a doubt Plato himself was, for Berkeley, the dominating figure in classical antiquity. Berkeley pays Plato the noble tribute, ' whose writings are the touchstone of a hasty and shallow mind ; whose philosophy has been the admiration of ages ; which supplied patriots, magistrates, and lawgivers to the most flourishing states, as well as fathers to the Church, and doctors to the Schools ' (332), and he wishes to lessen ' the prejudice that now lies against him ', and to commend ' those exalted notions and fine hints that sparkle and shine throughout his writings ' (338). But admiration is not acceptance. The man whose youthful thinking did not defer unduly to Locke or to Malebranche, the man who wrote in middle life, ' I have always thought and judged for myself ',[1] was not likely to take to himself a master in his old age.

Berkeley nowhere subscribes to Platonism ; he writes sympathetically of it, but from without. He says that, ' The Pythagoreans and Platonists had a notion of the true system of the world ' (266), and he proceeds to give an important summary of their principles, of which he evidently approves many, but not quite all. The Platonic ideas are not mentioned in this summary, but are discussed in sections 335-338. Berkeley does not indicate his own attitude towards them with any precision. He says that Aristotle has misrepresented them, but he is at no great pains to establish the true interpretation of them. I think he wanted their evidence for the supersensible and the support they provide for morality ; but I do not think he wanted them, or, indeed, could have found a place for them in his own metaphysic. He points out that, ' In Plato's style, the term *idea* doth not merely signify an inert inactive object of the understanding, but is used as synonymous with *αἴτιον* and *ἀρχή*, cause and principle '. Impersonal active causes could not possibly be fitted into Berkeley's philosophy, however it is interpreted.

There is then no evidence for the common belief that Berkeley in his old age modified his system in the direction of Platonism.

[1] *Defence of Free-thinking*, p. 19.

There is no *new* Platonism in the *Siris;* but is there any Platonism in the *Principles?* Berkeley did not *become* a Platonist in his old age; but was he in any sense a Platonist in his youth? The answer to this subsidiary question turns on the meaning of the term *archetype*, and the place of the archetype in the philosophy of the *Principles*. I have examined the question in detail in my *The Philosophical Correspondence between Berkeley and Johnson*, *Hermathena*, No. LVI (1940), pp. 99 ff., where I have shown that some passages accept archetypes as a legitimate hypothesis, but that other passages are scarcely compatible with a serious doctrine of archetypes. I conclude that Berkeley has left it an open question. Either he had not made up his mind, or, as is more probable, he wished to conciliate the English Platonists by conceding the possibility of archetypes in the mind of God, akin to the Platonic ideas. But his personal view, I think, and the view most consonant with his system, is that archetype and ectype meet in one, as far as we men are concerned; for the objects we perceive by sense *are*, Berkeley teaches, the things which God created and sustains; our ideas of sense *are* the ideas in the mind of God, and there seems no room for archetypes. If I am right about archetypes, then there are no doctrines, distinctive of Platonism, in the Berkeleian philosophy. In any case, there is no Platonism in the *Siris* which was not already in the *Principles*.

The alleged development of the Berkeleian philosophy has thus been examined and exposed. It is a flight of fancy, comparatively modern, without basis in fact.

The development seen in Berkeley's philosophy by critics to-day I attribute partly to mere mistake and partly to a confusion between development in the philosophy and development in the philosopher. Berkeley himself developed, as we all do. As the years went by, the focus of his eye altered, his friends passed over, and his own sands ran low; naturally his interest in the supersensible deepened, and his eye rested habitually on the horizon of life. But these personal changes neither required, nor made, any change in his early philosophy of sense and spirit. That philosophy, his one and only philosophy, he constantly defended. As new editions of his books came out, he tried to improve their wording and their style; but he never retracted, never withdrew, never modified in any particular the doctrine of the *Principles*.

XUM

IV.—BERKELEY ON "ABSTRACT IDEAS".

By Monroe C. Beardsley.

I

There are three propositions which I hope to demonstrate by the present argument. I contend (in Section II) that Berkeley's attack on abstract ideas is not made wholly compatible with his atomistic sensationalism; (in Section III) that Berkeley does not provide or employ a single definite criterion for determining the limit of abstraction; and (Section IV) that the doctrine of abstract ideas furnishes no real support to Berkeley's argument against the existence of material substance independent of perception. By "the doctrine of abstract ideas" I mean "the theory that there are no abstract ideas". But what sort of an idea would be an "abstract idea"? It would be one which "we are told" [1] by its defenders is achieved by abstraction; therefore it is this process which we must first consider.

Abstraction begins with an experienced thing (that is, a collection of qualities or ideas), and the first step (i) of the process occurs when the mind makes one quality the object of special attention. If a particular thing T consists of qualities Q_1, Q_2, Q_3, we can, Berkeley holds, "distinguish" and "consider" only Q_1 and ignore the other qualities of T, though they are co-present with Q_1 in our experience.[2] But he denies that we can perform the second step (ii), that of "resolving" T "into its simple, constituent parts, and viewing each by itself, exclusive of the rest".[3] If Q_1 could be thus "singled" out [4] and "framed" by the mind in separation from the other qualities, it would be an abstract idea. But such an idea does not occur as an element in consciousness. It is to be noted that in the second step the abstract idea would be merely the original idea conceived in isolation; it would not be a *new* idea.

But we do not know precisely what the isolation must be in order that the idea may be truly abstract, and it is not easy to determine what Berkeley means by "exclusive of the rest". (*a*) If we understand these words literally, he *says* that the

[1] *Principles*, Introduction, section 7.
[2] *Ibid.*, Intro., sec. 16.
[3] *Ibid.*, Intro., sec. 7.
[4] *Commonplace Book* 141; ed. G. A. Johnston (London, 1930); hereafter cited as *CPB*.

abstracted quality Q_1 cannot occur as an element in consciousness unless the qualities Q_2 and Q_3 also occur with it (for these are "the rest"). To give an example, if **T** has the qualities *square*, *red*, and *moving*, then this *square* cannot occur later in consciousness unless this *red* and this *moving* accompany it. But Berkeley obviously does not mean this, since he admits that we are capable of "variously compounding and dividing" our ideas, and this rule would prohibit the imagining of a centaur [1] or a "blue horse".[2] (*b*) We might interpret him to mean that Q_1 cannot occur later without being accompanied by any idea at all; yet this would be too trivial for his purpose. (*c*) The best interpretation I can suggest then is the following one. Berkeley seems to mean that whatever qualities (including Q_2 and Q_3) may be absent from a later thought of Q_1, we cannot think of Q_1 without at the same time thinking of such other ideas as will, with Q_1, constitute a complex idea of a particular thing. That is, if Q_1 is originally perceived as part of a thing (**T**), then it can only be conceived later as part of a thing. To say that there is no abstract idea of a particular *red* is to say that there is no idea of that *red* apart from a thing. We may then define in a preliminary way the abstract idea of Type A: it is a *particular* idea perceived as a quality of a thing and later (supposedly) conceived without any thing which it qualifies.

The second type of abstract idea requires (iii) a third step, by which [3] abstract ideas of Type A are themselves compared with each other, so that from a given class of abstract ideas we frame "a most abstract idea" which is not merely abstract, but also general. That is, given Q_a (the abstract *red*) and Q_b, Q_c, other abstract ideas, *blue*, *green*, *yellow*, etc., the mind frames an "abstract general idea", *colour*, which is not any particular colour, but is all and none of them. In this case there is a "twofold abstraction": [4] the particular colours are abstracted from particular objects, and then the general idea *colour* is abstracted from classes of the particular colours. It is to be noted that the abstract ideas of Type B would be *new* ideas created by the mind; in the third step the mind does not merely select what is common to all particular colours, but forms an idea of *colour* that is not one of the simple ideas perceived as part of a given coloured object. In the case of T, *colour* is not a "constituent" idea (like *red* or *square*) that could be "considered" and later segregated; it could never be abstracted from things save by comparison of an idea abstracted from a thing with

[1] *Prin.*, Intro., sec. 10.
[2] *CPB* 766.
[3] *Prin.*, Intro., sec. 8.
[4] *Ibid.*, sec. 99.

other abstract ideas. Therefore the formation of abstract general ideas of Type B would depend on and presuppose the prior formation of abstract ideas of Type A.

The abstract idea of Type B is still a simple idea, a quality; that of Type C is a complex idea of a "being", such as *man* or *humanity*. In this case "several co-existent qualities" of each thing[1] are abstracted (as in Type A), then each complex of qualities is compared with others (as in Type B), and another idea is formed which is in effect a complex of ideas of Type B, and like them is a *new* abstract general idea. Now it is, I think, fairly obvious that Berkeley's attack on abstract ideas is specifically directed against abstract *general* ideas (of Types B and C); at them Berkeley delivered his "killing blow".[2] For (*a*) they are the ones which are "contradictory"[3] and "inconsistent" (Locke's term), and (*b*) Berkeley regarded his theory of signs as supplying all that was required of these abstract general ideas, namely, a theory of the manner in which particular ideas can become general. But he saw that ideas of Types B and C are formed upon the prior step (ii), which yields ideas of Type A; and when he came to examine the ideas of Type A he rejected them as well. He rejected them, not because our ability to frame ideas of Type A implies our ability to frame ideas of Types B and C (that is clearly not the case), but because the purpose to which he decided to put his critique of abstraction seemed to require that he reject the first type as well. At an earlier stage of thought[4] Berkeley had noted that "'Tis one thing to abstract one concrete idea from another of a different kind, & another thing to abstract an idea from all particulars of the same kind", thus recognising the distinction between Type A and Types B and C. But in the *Principles* both are condemned.

Berkeley sometimes writes, in giving illustrations of abstract ideas, as though he meant that there are certain *abstract* ideas that cannot be abstracted from other *abstract* ideas, as when he says that the mind cannot "frame to itself by abstraction the idea of colour exclusive of extension".[5] But he is, of course, always referring to a particular *colour* (Q_1) and a particular *extension* (Q_2) in a particular thing, and saying that neither of these can become ideas of Type A. It is not that we cannot think of colour without thinking of extension, but that we cannot think of a particular *colour* without thinking of some particular *extension*. The doctrine of abstract ideas can then

[1] *Prin.*, Intro., sec. 9. [2] *CPB* 699. [3] *CPB* 566.
[4] *CPB* 499. [5] *Prin.*, Intro., sec. 7.

be expressed in this precise form: there are certain classes of ideas such that no member of one class can be thought of at a given time unless at least one member of the other class is thought of at the same time. To say that shape cannot be abstracted from colour is to say that if idea x is a member of C_s (the class of all particular shapes) and x occurs as an element of consciousness, then there will be some idea y such that y is a member of C_c (the class of all particular colours) and y accompanies x. The phrase "element of consciousness" is meant to include in its denotation thoughts of all sorts (images, concepts, ideas). For Berkeley would not merely hold that x could not be an image without y; he would say what he says of extension: "I do not find that I can perceive, imagine, or anywise frame in my mind such an abstract idea".[1]

II

With the above preliminaries we can approach the first question: that concerning the relation of the doctrine of abstract ideas to the general framework of Berkeley's epistemology. From this point on, when I speak of "abstract ideas" I shall be referring only to ideas of Type A. Those ideas become abstract when they are separated from things. But when we examine Berkeley's conception of what constitutes a thing, it becomes something of a problem to decide why these ideas should not be separable from things. Berkeley's view of perception does not seem to permit, much less necessitate, his doctrine of abstract ideas.

The difficulty may be stated thus. Berkeley's definition of the word "thing"—exclusive of spirits, which come under the most general range of "thing"[2]—is given in the first section of the *Principles* (Part I). My senses, he says, furnish me in their separate ways with various ideas, "and as several of these are observed to accompany each other, they come to be marked by one name, and so to be reputed as one *thing*".[3] A "sensible thing" is a "collection" or "congeries"[4] of ideas "blended or combined together".[5] When the ideas first appear to the mind they come through various senses, by various channels; they are distinct and *several*, for they are observed severally to go together, and they may be "apprehended by divers senses,

[1] *Theory of Vision*, sec. 123.
[2] See *Prin.*, sec. 89, and *CPB* 653.
[3] *Prin.*, sec. 1.
[4] *Three Dialogues between Hylas and Philonous*, III; in the Selections ed. by Mary W. Calkins, Scribner's Modern Student's Library (N.Y. 1929), p. 324.
[5] *Prin.*, sec. 3.

XUM

or by the same sense at different times, or in different circumstances".[1] Then they are "united into one thing by the mind".[2] It is the mind which gives them a substantial unity, and a name, and the mind is led to do this, not by any intrinsic relation among the ideas, but by extrinsic spatial and temporal relations; men, says Berkeley, select those collections of ideas which are "observed, however, to have some connexion in nature, either with respect to co-existence or succession; all which they refer to one name, and consider as one thing".[3]

If this is a true account of the manner in which Q_1, Q_2, and Q_3 came to constitute T, then we may ask with some surprise why the mind cannot abstract Q_1. Why can there not be abstract ideas of Type A? If things are "*arbitrarily* put together by the mind",[4] they should perhaps be arbitrarily separable by the same mind; if the ideas originally entered the mind as separated and discrete, it is strange that the mind should so tightly have bound them together that they can never occur again in the mind as separated and discrete. There are two explanations of this phenomenon which might be urged on Berkeley's behalf. (1) It might be said that there is an intrinsic and necessary connection between ideas as they are perceived and that the mind is to some extent guided by insight into this connection, in learning about objects. Then certain sorts of abstraction would be impossible. But Berkeley holds that we cannot discover any necessary connection between our ideas;[5] their only connections are adventitious and external. We know, and need only know, relations of "co-existence and succession" in order to form things.[6] As for *a priori* principles—such as one asserting a necessary connection between colour and extension—these principles (since they would have to be expressed in abstract terms) would be particularly abhorrent to Berkeley's basic position. (2) It is true that in the case of some of the rejected abstract ideas Berkeley could say that these cannot be *abstract* ideas because they are not ideas at all. This is true of abstract general ideas like *colour* and *humanity*, as I have explained above. What is not perceived as one of the distinct qualities of T naturally cannot later be abstracted from T and conceived as a separate quality. In this way Berkeley can account for our inability to abstract *some* ideas, and if he were to apply this account to all ideas that cannot be abstracted, he

[1] *Three Dialogues*, III, p. 319.
[2] *Ibid.*, III, p. 320; see also p. 324.
[3] *Ibid.*, III, p. 319.
[4] *Prin.*, sec. 12.
[5] *Ibid.*, sec. 31; see also *CPB* 896.
[6] See *CPB* 752.

would be adopting Hume's clearly-stated maxim, "all ideas which are different are separable". But it appears that, though Berkeley wavers on this point, he does not ultimately intend to maintain this general principle; and in certain crucial cases he does not apply it.

As an example we may take motion. Berkeley sometimes speaks as though a particular *motion* is an idea we acquire through sight, but an idea that is not abstractable from particular *extensions* and *shapes*.[1] Now Berkeley also says that every *motion* includes relation,[2] and relations are not ideas at all.[3] But whether a *motion* be "not one idea",[4] or "on 2d thoughts" a "simple idea",[5] Berkeley never definitely suggests that its unabstractability is a consequence of its not being an idea at all. But this is not the case with extension. First we must note Berkeley's statement that "the mind, 'tis true, can consider one thing without another; but then, considered asunder, they make not 2 ideas. Both together can make but one, as for instance colour & visible extension."[6] This seems to mean that in perception we cannot even *distinguish* or *discriminate* two ideas, *extension* and *colour*, as particular elements of a plane figure. Here Berkeley verges close upon Hume's position. For though we may, he holds, distinguish between two kinds of visible extension, the mathematical and the common,[7] the former may be defined as simply the co-existence of point-perceptions;[8] and the latter[9] is not a visual idea at all, Berkeley says in some places, for there is no "idea intromitted immediately and properly by sight save only light and colours".[10] Thus, in a particular object **T**, the shape *square* is, on this hypothesis, not an idea at all: "in a strict sense, I see nothing but light and colours".[11] Now if this is Berkeley's position, then it would explain why colours and shapes, for example, cannot be abstracted: because they are the same idea. But Berkeley does not consistently adopt this standpoint; it is more in accord with the general position of the first part of the *Principles* when we find him referring to the *extension*, *colour*,

[1] See *CPB* 877, 449, and also *Free-Thinking in Mathematics*, sec. 20.
[2] *Prin.*, sec. 112. [3] *Ibid.*, sec. 89. [4] *CPB* 188.
[5] *CPB* 448. [6] *CPB* 330. [7] *CPB* 396.
[8] *CPB* 295, 437, passages worth careful comparison with Hume's treatment of space and time. Berkeley adds that extension may also be defined as "length without breadth", which in some places he calls an "abstract" idea (*CPB* 263, 485, 385), and in other places no idea at all (*CPB* 384). In this last passage Berkeley significantly suggests that "It seems to consist in meer proportion—meer reference of the mind".
[9] *CPB* 459. [10] *Theory of Vision*, sec. 130. [11] *Ibid.*

and *motion* as "simple, constituent parts" of a particular object,[1] or when we find him speaking of "a certain colour, taste, smell, figure and consistence having been observed to go together",[2] which could not occur if they were not discriminable ideas. Finally, we may observe that if two particular ideas are unabstractable because they are identical, then neither can be "considered" in itself, and Berkeley's explanation of the manner in which we come to make words and thoughts general presupposes our ability to consider the particular ideas which Berkeley holds to be unabstractable. For these reasons I submit (*a*) that in both the *Principles* and the *Dialogues* Berkeley regards the particular unabstractable ideas as *genuine* ideas, and (*b*) that he offers no explanation of the impossibility of abstraction in these cases.

III

I have presented reasons for asserting that Berkeley's doctrine of abstract ideas derives no justification from his theory of perception and is even in contradiction with it. But even if we were to grant that he is right in holding that there are some ideas which are not abstractable, we would still have to enquire how we may know *which* ones are not abstractable. The obvious reply to this is an appeal (which Berkeley often makes) to immediate experience; but since his whole purpose (that of removing ancient prejudices about abstract ideas) presupposes that the common run of philosophers have misinterpreted their own experience, this appeal does not suffice.[3] Berkeley must provide a principle by which abstractable ideas are distinguished from non-abstractable ones, and by which the difference between them is made clear. We may put the question in other words: if qualities cannot be conceived apart from things, how do we know which collections of ideas may be things? What defines the limit of abstraction?

To this question Berkeley has an answer, but it is not a univocal one. "I do not deny it [the mind] may abstract in a certain sense", says Euphranor,[4] "inasmuch as those things

[1] *Prin.*, Intro., sec. 7. [2] *Ibid.*, sec. 1.

[3] Berkeley tacitly admits this, for example, when in writing to Samuel Johnson about the abstract idea of existence, he remarks, "I cannot find I have any such idea, and this is my reason against it . . ." (24 March, 1730; given in *Works*, ed. A. C. Fraser, Oxford, 1901; II, 20); see also *CPB* 615.

[4] *Alciphron*, VII, 5; *Works*, II, 328.

that can really exist, or be really perceived asunder, may be conceived asunder, or abstracted one from the other." One interpretation to which this passage lends itself is, I submit, the following. A person may abstract those, and only those, ideas which experience indicates to be separable in fact, or to belong to classes of ideas which are separable in fact. If I have perceived at least one particular *shape* without any sort of *motion* at all, then I may later conceive any particular *shape* without conceiving of any *motion* in it. Conversely, if every particular *motion* I have perceived has been accompanied by some *shape* (that of the thing moved), then I cannot abstractly conceive any particular *motion* without conceiving along with it some *shape*. This interpretation I shall call Principle A, defining it as follows: The ideas of Class C_1 are abstractable (in the sense of Type A) from the ideas of Class C_2 if and only if (i) there is an idea x such that x is a member of C_1, and x was perceived at time t, and (ii) there is no y such that y is a member of C_2, and y was perceived at time t, conjoined with x. To illustrate this Principle: it would imply that motions are not abstractable from shapes, but shapes are abstractable from motions; that colours and shapes are not abstractable from each other; and that, if I have perceived, for example, an electric light bulb apart from a socket, then I may conceive any electric light bulb apart from its socket. So far, Principle A seems to suit Berkeley's purpose and to represent his meaning faithfully.

But if it is closely examined, difficulties appear. "I can", says Berkeley,[1] "consider the hand, the eye, the nose, each by itself abstracted or separated from the rest of the body"; and he evidently expects us to be able to perform the same feat. Similarly, he says that he is able to conceive a human head without a body, a human trunk without limbs, and a rose's smell without a rose, though he has not, he implies, perceived the members of these pairs separately. For, he reports, "I may, indeed, divide in my thoughts, or conceive apart from each other, those things which perhaps I never perceived by sense so divided".[2] Therefore Principle A is really too narrow for Berkeley's purpose; it limits abstraction too much and leaves too little freedom for the imagination. Moreover, Principle A involves a relativity for which there is no room in Berkeley's system. It must be stated in terms of every individual person's own experience, which bounds his ability to conceive abstractly;

[1] *Prin.*, Intro., sec. 10. [2] *Ibid.*, sec. 5.

XUM

and as far as Berkeley's own experience goes he can therefore never be certain (as he appears to be) that Locke and Hylas have never experienced any colour without extension or any motion without a thing that moves.

Therefore Principle A is not enough for Berkeley; and indeed there is another interpretation of the *Alciphron* passage which seems to represent Berkeley's intention more truly. The limit of abstraction, according to this principle, depends not on what actually has occurred in experience, but on what may *possibly* occur. That is, abstraction "extends only to the conceiving separately such objects as it is possible may really exist or be actually perceived asunder".[1] This principle is, of course, wider than Principle A, for it permits us to abstract what has not been perceived in separation but might be perceived in separation (such as the human nose or the human eye). We then may define Principle B as follows: The ideas of Class C_1 are abstractable from the ideas of Class C_2 if and only if it is *not impossible* that the following is true: (i) there is an idea x such that x is a member of C_1, and x is perceived at time t, and (ii) there is no y such that y is a member of C_2, and y is perceived at time t, conjoined with x.

The trouble with Principle B is that it does not help us to decide just what ideas are abstractable, and what not. For how are we to know which combinations of ideas (objects) are possible or impossible? Experience, Berkeley admits, reveals no such absolute necessities. The only way we know (if indeed we do know) that colours and shapes cannot *possibly* ever be perceived or exist in separation is by consulting our own minds and asking whether we can conceive them to be perceived or to exist separately. But if we decide whether they can be conceived abstractly by determining whether they can exist separately, and then decide whether they can exist separately by determining whether we can conceive them to exist separately, and then decide whether we can conceive them to exist separately by determining whether we can conceive them separately, then our decision in any particular case is thoroughly circular.[2] The only escape from this circularity would be to say that they cannot be conceived separately because they have not in fact existed in separation; but this makes Principle B equivalent to Principle A. And in the absence of this conversion of Principle B into Principle A, Principle B really is

[1] *Prin.*, Intro., sec. 5; see also Intro., sec. 10.

[2] This circularity is perhaps even more evident in that passage from the *Alciphron*, VII, 6, omitted from the 3rd ed. (1752); see *Works*, II, 325 n.

not an informative principle at all. But Principle A seems to be both doubtful and too narrow for Berkeley's argument.

IV

At various points in his refutation of realism Berkeley insists (*a*) that the prevalence of this erroneous belief among philosophers has chiefly been due to their uncritical assumption that we possess abstract (and abstract general) ideas, and (*b*) that the untenability of the belief is clearly exposed when we realise that we do not possess abstract ideas. There is no doubt that Berkeley regarded his doctrine of abstract ideas (as well as his doctrine of abstract general ideas, which I shall not discuss) as an essential ground of his immaterialism. So insistent is he on this point that we should expect the connection between his idealism and his doctrine of abstract ideas to be quite plain. There are, however, fatal obscurities in this connection, and the more seriously one considers them the more significance one discovers in Hume's position in the first and second parts of the *Treatise*, Book I.

The provoking situation we encounter in trying to trace this connection is that Berkeley turns out (even at the early stage of development represented in the *CPB*) to be employing the word "abstract" as a synonym for "non-existent" and "unworthy"; and when he caps an argument for idealism with a poke at abstraction, we readily discover, in some instances, that abstraction has nothing to do with the case. For example, he declares that the object T cannot exist apart from the sensation of it, for the reason that T cannot be abstracted from the sensation of it. But his statement[1] that "the object and the sensation are the same thing and cannot therefore be abstracted from each other" seems to mean only that T cannot be abstracted from Q_1, Q_2, and Q_3, since it is by definition just the combination of them. But since T is just its combined qualities (which are ideas), then T cannot even be *distinguished* from them (by step i), let alone *abstracted* from them (by step ii), so it is difficult to see how Berkeley's doctrine that there are ideas which are distinguishable but not abstractable (that is, which may be "considered" but not "separated") is relevant to the present case. Moreover, it may be a fact that "when we do our utmost to conceive the existence of external bodies, we are all the while only contemplating our own ideas",[2] but

[1] *Prin.*, sec. 5. [2] *Ibid.*, sec. 23.

if we cannot do more than this, the important question arises: how did we come to think we could?

We seem to be opening a more fundamental vein of Berkeley's argument when we examine his statement that, "as it is impossible for me to see or feel anything without an actual sensation of that thing, so is it impossible for me to conceive in my thoughts any sensible thing or object distinct from the sensation or perception of it".[1] This assumes a shape different from that of the argument just considered, and at first seems clearly to turn on the doctrine of abstract ideas. We believe (*a*) that we can conceive a given object T as existing apart from the perception of it by someone; we make this mistake because we think on superficial examination (*b*) that we can have an idea of T existing without having any idea that T is being perceived. But the fact is (*c*) that we have never perceived T without perceiving it, and this fact (*c*), by Principle A, makes proposition (*b*) false. Our belief (*a*) is therefore ungrounded, and it follows from a denial of (*a*) that T cannot exist apart from our perception of it.

Now it is clear that the crucial point of this argument is the demonstration that the idea *T existing* cannot be abstracted from the idea *T being perceived*.[2] When we look about for Berkeley's demonstration, we discover that he very often says quite explicitly (1) that the reason is, that "the existence of an idea consists in being perceived"; [3] that, in short, the ideas are the same.[4] To be precise, we must heed his warning: "But it must be well noted that existence is vulgarly restrain'd to actuall perception, & that I use the word existence in a larger sense than ordinary".[5] We can then express this view by the proposition that "T existing" is precisely identical in meaning to "T being thought of" in the widest sense (T_{ex} is T_{th}): "This I am sure", remarks Berkeley, "I have no idea of Existence, or annext to the word Existence".[6] That is, there is no unique idea of existence apart from perception: "'Tis on the discovering of the nature & meaning & import of Existence that I chiefly insist".[7] According to this argument, then, "esse est percipi"

[1] *Prin.*, sec. 5; see also *Three Dialogues*, I, pp. 261-262.

[2] Since the idea *T, being perceived* includes reference to a self, which is a *notion*, the idea is perhaps more accurately called a notion. This would make no significant difference as far as the argument is concerned, I believe, since Berkeley holds also that we have no "abstract notion" of the powers and acts of the self apart from the self or apart from the self's ideas (*Prin.*, sec. 143).

[3] *Ibid.*, sec. 2.

[4] See *ibid.*, sec. 3; *CPB* 404.

[5] *CPB* 472; see also 471.

[6] *CPB* 681; see also 680, 557.

[7] *CPB* 493.

is analytic; I have no *abstract* idea of T_{ex} for the same reason that I have no abstract idea of *colour:* it is not an idea at all.

Now this is a perfectly good argument, if it *is* Berkeley's argument. What is noteworthy about it is that it owes nothing to the doctrine of abstract ideas. T_{ex} is so far from being an abstract idea apart from T_{th} that it is the same idea; not only can it not be abstracted (by step ii) from T_{th}, but, being identical with T_{th}, it could not (by step i) even be "singled out" from T_{th}—indeed, it could not even be "distinguish'd therefrom", as Berkeley admits.[1] If Berkeley's argument here is valid, then there could indeed be no "nicer strain of abstraction than to distinguish the existence of sensible objects from their being perceived, so as to conceive them existing unperceived".[2] Indeed one "might as easily divide a thing from itself", for that is precisely what one would be doing. But if the conclusion is that "to exist is to be perceived" is analytic, and for that reason true, it also follows that no one would even distinguish existence from perception, and therefore that no one could have been misled by the belief in abstract ideas to think that it is possible to conceive T_{ex} in abstraction from T_{th}.

As a matter of fact, Berkeley does not confine himself to the argument that "esse est percipi" is analytic; he also (2) holds to the position that it is synthetic. And this is quite incompatible with the other position, for in this case T_{ex} *is* an idea, distinct from T_{th}, but inseparable from it.[3] This argument does depend upon the doctrine of abstract ideas, as a statement of its steps discloses: (*a*) When T is perceived it is accompanied by the idea (T_{th}) that T is being thought of by someone; (*b*) no T has ever been perceived without being accompanied by such an idea; by (*c*) Principle A it follows that (*d*) T cannot later be conceived without T_{th}; therefore (*e*) T_{ex} (since it includes T) cannot be conceived without T_{th}, and hence (*f*) T cannot exist apart from being thought of.

We seem, then, to have discovered the point at which Berkeley applies his doctrine of abstract ideas to the refutation of realism. Step (*f*) of the above succinct version of his argument depends upon considering T_{ex} as an idea distinguishable, but not abstractable, from T_{th}. And yet it seems clear that Berkeley himself has provided his own answer to this argument. For we may ask whether steps (*d*) and (*e*) are in fact correct; "surely there is nothing easier than for me to imagine trees, for instance, in a park, or books existing in a closet, and nobody

[1] *CPB* 655. [2] *Prin.*, sec. 5. [3] See *ibid.*, sec. 6.

XUM

by to perceive them". To this Berkeley assents,[1] but he considers it irrelevant. It is no objection to his argument, he declares, to say that I can frame the idea of things existing "at the same time omitting to frame the idea of any one that may perceive them". This is only, he says, a case where "the mind, taking no notice of itself, is deluded to think it can and does conceive bodies existing unthought of, or without the mind, though at the same time they are apprehended by, or exist in, itself".[1] But (and this is the crucial question with respect to steps (*d*) and (*e*)) is not this possibility (which Berkeley here admits) that T_{ex} may occur in the absence of T_{th} (*i.e.*, that we may frame T_{ex} and omit to frame T_{th}) precisely what the doctrine of abstract ideas (as we have defined it in Section I above) denies? In the case of colour and shape, Berkeley strives to prove just this: that we cannot think of a colour without thinking of some shape. If we apply this to the present case, his argument must be that we cannot think of any particular thing without thinking that it is being thought of by someone. And yet in order to explain how philosophers have been tricked into realism by their belief in abstract ideas, Berkeley must (as in the passage above) contradict his general principle by saying that we can think of a particular thing without thinking that it is being thought of.

There is one more instance of Berkeley's use of the doctrine of abstraction which requires notice here. At one time he regarded the inseparability of the primary from the secondary qualities as "the great argument"[2] against materialism. According to this view the erroneous belief that extension and motion can be abstract ideas apart from colours has been chiefly responsible for the error of realism;[3] and when it is realised that we cannot conceive extension apart from colour, it will be understood that extension cannot exist apart from colour; and since the latter is admittedly subjective, so also must the former be.[4] In view of all the preceding discussion this does not call for much comment. For if the proposition (*a*) that I have no abstract idea of extension apart from colour is offered as ground for the proposition (*b*) that extension cannot exist apart from colour, this inference can only be effected by Principle B, as discussed in Section III. But Principle B cannot be applied without either begging the question, or being converted into Principle A; and Principle A will not permit the inference of proposition (*b*), but only of the proposition (*c*) that I have never

[1] *Prin.*, sec. 23. [2] *CPB* 297. [3] *CPB* 380.
[4] *Prin.*, sec. 10; see also *Three Dialogues*, I, p. 253.

in fact experienced extension apart from colour. This is not to say that Berkeley's discussion of the relation between primary and secondary qualities amounts to no more than this; it is only to say that, as far as the doctrine of abstract ideas is concerned, the following conclusions seem to be warranted: (1) the doctrine appears to furnish less actual support to Berkeley's system than Berkeley supposes, and (2) it suffers from certain inner difficulties which cannot be removed without changing it in such a way that it furnishes no support at all.

XUM

V.—DISCUSSION.

IMPRESSIONS AND IDEAS: A NOTE ON HUME.

When philosophers get down to business it is often quite plain that their meaning enlarges or even contradicts their express declarations about basic principles. When this happens their practice may be better than their declarations or it may not. That, however, is not a sufficient reason for by-passing the said declarations.

Hume's doctrine of impressions and ideas—his "articles of inquisition" as Reid called them—seem to me to be an instance of the above elementary truism. I am thinking particularly of the valuable work of Mrs. Maund and of the invaluable work of Mr. Kemp Smith. It does seem to me that these authors, rightly challenging many of the traditional interpretations of Hume, are inclined to evade certain of his most emphatic pronouncements.

Mrs. Maund in sheer error (which is very unlike her) says that the "so-called principle of the Inquisition" was "Reid's invention and has no application at all in Hume's philosophy" [M. 210].[1]

Let us hear Reid [*Inquiry* in *Works*, Hamilton's ed., 144]:

"The articles of inquisition are few indeed but very dreadful in their consequences; Is the prisoner an impression or an idea? If an idea, from what impression copied? And if it appears that the prisoner is neither an impression, nor an idea copied from some impression, immediately, without being allowed to offer anything in arrest of judgment, he is sentenced to pass out of existence and to be, in all time to come, an empty unmeaning sound or the ghost of a departed entity. Before this dreadful tribunal, cause and effect, time and place, matter and spirit, have been tried and cast."

Now let us hear Hume:

"All the perceptions of the human mind resolve themselves into two distinct kinds which I shall call impressions and ideas" [T. 1. Thus the *Treatise* begins. In the Advertisement to Book III, which was published separately (a year later), the only philosophical instruction given to a virgin reader was to note the cardinal distinction between ideas and impressions.]

"No discovery could have been made more happily for deciding all controversies concerning ideas than that above mentioned that impressions always take the precedency of them, and that every idea, with which the imagination is furnished, first makes its appearance in a correspondent impression" [T. 33].

[1] In what follows M. = *Hume's Theory of Knowledge*, by C. Maund; K. = *The Philosophy of David Hume*, by N. Kemp Smith; T. = Hume's *Treatise*, in Selby-Bigge's edition; E. = Hume's *Enquiry*, also *apud* Selby-Bigge.

" We shall always find that every idea which we examine is copied from a similar impression. Those who would assert that this position is not necessarily true nor without exception have only one and that an easy method of refuting it: by producing that idea which, in their opinion, is not derived from this source " [E. 19].

" When we entertain therefore any suspicion that a philosophical term is employed without any meaning or idea (as is but too frequent) we need but enquire, *from what impression is that supposed idea derived?* And if it be impossible to assign any, this will serve to confirm our suspicion. By bringing ideas into so clear a light, we may reasonably hope to remove all dispute, which may arise, concerning their nature and reality " [E. 22].

It is difficult to see how Reid's spirited comments could be more amply justified, but, for better measure, it is worth remarking that Hume applied his instruments of torture just where Reid said he did (as well as extensively elsewhere). The whole of Hume's elaborate enquiry into cause and effect in the *Treatise* is in form [" to begin regularly," T. 74] a search for the impression from which the idea of necessity [1] is derived. When the torture-chamber is eventually reached in Sect. xiv, the inquisitorial thumbscrews are expressly and repeatedly applied [*e.g.* T. 155: " as we have no idea that is not derived from an impression we must find some impression that gives rise to this idea of necessity if we assert that we really have such an idea "]. Regarding " time and place " Hume said, *inter alia*, " If it be a sufficient proof that we have the idea of a vacuum because we dispute and reason concerning it, we must for the same reason have the idea of time without any changeable existence; since there is no subject of dispute more frequent and common. But that we really have no such idea is certain. For whence should it be derived? Does it arise from an impression of sensation or of reflexion? Point it out distinctly to us that we may know its nature and qualities. But if you cannot point out *any such impression* you may be certain that you are mistaken when you imagine you have *any such idea* "[T. 64 f.]. Concerning matter (= the idea of external existence) Hume said [T. 67], " Nothing is ever really present with the mind but its perceptions, or impressions and ideas. . . . Now since nothing is ever present to the mind but perceptions, and since all ideas are derived from something antecedently present to the mind, it follows that 'tis impossible for us so much as to conceive or form an idea of anything specifically different from ideas and impressions." Of spiritual substance he said [T. 232], " I know no better method than to ask these philosophers in a few words what they mean by substance and inhesion. . . . As every idea is derived from a precedent impression, had we any idea of the substance of our minds we must also have an impression of it." Of the self he said [T. 251 f.], " There are some philosophers who imagine we are every moment intimately con-

[1] This particular idea was ' tried ' but *not* ' cast ' in *all* its senses.

scious of what we call our Self. . . . Unluckily [for them we have no] idea of self after the manner it is here explained. For from what impression could this idea be derived? This question 'tis impossible to answer without a manifest contradiction and absurdity; and yet 'tis a question that must necessarily be answered if we would have the idea of self pass for clear and intelligible. It must be some one impression that gives rise to every real idea. . . . [And after some argument.] It cannot therefore be from any of these impressions or from any other that the idea of self is derived, and consequently there is no such idea."

Mr. Kemp Smith, who quotes (for a rather special purpose) the two passages from the *Enquiry* that I have cited above, says about them [K. 218], "The apparently final, decisive character of the doctrine which Hume expounds in these opening sentences of the *Treatise* is a main reason why the traditional view of Hume's teaching has gained such general currency. For not only has Hume abstained from complicating his argument by any premature reference to doctrines which do not at this stage allow of sufficiently clear statement; he has, less happily, also abstained from conveying any warning to his readers that the analysis of experience which he is here giving is very far from complete, and that in addition to impressions and ideas—the materials of experience, which are alone here mentioned—they will be called upon to recognise types of 'object' quite other than impressions and ideas, *viz.* the objects of knowledge and the objects of belief, and as conditioning these two types of object, certain further factors such as 'acts of comparison', 'propensions' of the imagination, 'qualities of human nature'."

These statements invite adverse comment. I submit that "the apparently final decisive character" of these statements *was*, in Hume's opinion, "final and decisive". When he departed from them (as we must all allow he did) he never admitted that he did. I have also shown above (I think very fully) that the statements, so far from being an introductory flourish later to be drastically revised, were repeatedly reaffirmed and most deliberately used at critical points throughout the entire first book of the *Treatise*.

Among the passages I have quoted above from Hume one [E. 22] states that a philosophical term is used "without any meaning or idea" if it cannot be shown to copy an impression. The same thing is said, with equal clearness, in the vacuum passage I have quoted and in the passage I quoted about the "self".

Despite these and other similar statements of Hume's, Mrs. Maund and Mr. Kemp Smith believe themselves entitled to say something very different. Mrs. Maund says [M. 165], "In Hume's investigation . . . if there is anything about which any question may be asked then, *ipso facto*, there is that accusative", and again [M. 210], she says it is one of Hume's most important characteristics that "whenever we use words we refer to something, and whenever we say

sentences we say something, so that the very fact that a problem is raised shows that it must be a problem about something and not about nothing". In a fashion somewhat similar Mr. Kemp Smith says, "It can indeed be maintained as a general principle that Hume never denies the existence of any conception which has been the subject of controversy. . . . The fact that there has been controversy in regard to an idea shows, he holds, that the idea is there to be discussed" [K. 254].

Where did Hume "hold" this? Is it really a "general principle" for the interpretation of the man who said (as we saw) that it was "but too frequent" for a "philosophical term to be employed without any meaning or idea", who complained of the "unintelligible chimera of a substance" [T. 222], who, when he twitted the exponents of "spiritual and refined perceptions", told them that their vice "ran through most parts of philosophy" and "covered many of their absurdities" [T. 72]? Is it seriously to be disputed that Hume would gladly have seen incinerated an enormous number of pages of philosophy on the ground that, in his opinion, they consisted of verbiage about verbiage? I am not disputing the important circumstance that much of Hume's most valuable work consists in his sedulous attempts to transform pseudo-problems into genuine ones, and, if possible, verifiable ones. He may have held that philosophers did not differ very often unless there was *something* to be discussed. Nevertheless, he emphatically denied the existence of very many "conceptions which have been the subject of controversy". What we can collect about his "general principle" in this matter I think should be inferred from statements like the following [T. 162]: "When we talk of any being . . . as endowed with a power or force . . . when we speak of a necessary connexion betwixt objects . . . in all these expressions *so apply'd* we have really no distinct meaning and make use only of common words without any clear or determinate ideas. But as 'tis more probable that these expressions do here lose their meaning by being *wrong apply'd* than that they never have any meaning; 'twill be proper to bestow another consideration on this subject to see if possibly we can discover the nature and origin of these ideas we annex to them." It does not seem to me that from any such passage we can fairly elicit any such principle as Mr. Kemp Smith lays down.

I shall end this discussion by mentioning an addendum on a point of some interest. It is only an addendum, and is not offered in support of anything that has preceded it. A consistent reader (if I have any) could reject what I am about to say, while accepting what I have said before, without any strain upon his intellectual conscience.

Mr. Kemp Smith says [K. 205, n.], "Hume has also spoken of ideas as 'representing' the correspondent impressions and some of his critics have taken this as showing that he at times regards

them as ideas *of* their impressions, *i.e. meaning* them, *referring* to them. I agree with Dr. Constance Maund that there is no evidence that Hume ever thought of so regarding them."

I take it that the dispute here is not about the question whether an idea represents *the particular* impression from which, in the first instance, it was derived. On that point Hume was sufficiently explicit [T. 84]: "I need not observe that 'tis no just objection to the present doctrine that we can reason upon our past conclusions or principles without having recourse to those impressions from which they first arose. For even supposing these impressions should be entirely effaced from the memory, the conviction they produced may still remain."

This being understood, Mr. Kemp Smith's statement, strictly interpreted, asserts that a representation does *not* mean or refer to what it represents. I find this hard to believe.

Mrs. Maund [M. 90] speaks about Hume's "unfortunate remark" [at T. 157] that "ideas always represent their objects or impressions". If "unfortunate", the remark is not singular—"Simple ideas" at T. 17 are said to "represent qualities". Of "ideas" more generally we read, at T. 7, "more lively perceptions from which they are derived and which they represent". At T. 19 we have the same thing twice, *viz.* "copies and representations of them", "its copy or representative". At T. 37 we have the emphatic statement, "Ideas always represent the objects or impressions from which they are derived and can never without a fiction represent or be applied to any other".

This looks to me very like evidence. What is the prodigious difference between "meaning and reference" on the one hand, and "representation and application" on the other?

Consider some other terms that Hume used as equivalent. At E. 17 ideas were said to "mimic or copy" impressions. At T. 165 a certain impression of reflection was said to be the "real model" of "the idea of necessary connection". Hume also used the term "image of". Thus at T. 6 he said, "as our ideas are images of our impressions, so we can form secondary ideas which are images of the primary; as appears from this very reasoning concerning them". (Is 'reasoning concerning' not 'meaning and reference'?) There is no doubt at all that Hume sometimes thought of an image as referring or conveying meaning. Thus at T. 189 we read, "That our senses offer not their impressions as the images of something distinct or independent and external is evident; because they convey to us nothing but a single perception and never give us the least intimation of anything beyond". [Even if "convey" here is ambiguous, "intimation" is not.]

In the language of the seventeenth and eighteenth century the word "original" commonly stood for a logical source (or standard) as well as for a historical source (or mere beginning). For Hume, impressions were originals, at any rate *quoad nos*. Ideas were only

copies. It is also to be remarked that Hume did not discard the copy theory of truth (*e.g.* T. 415: "'Tis impossible that this passion can be opposed by or be contradictory to truth or reason; since this contradiction consists in the disagreement of ideas considered as copies with these objects which they represent".)

In ordinary English usage the "of" in "idea of"—Hume's habitual term—is referential. I can see no reason for supposing that, for him, "to conceive or form an idea of" means anything different from "thinking of", and I can see very little point in his insistence that an idea not derived from an impression had *no* meaning (or no "real" meaning) unless he meant that the "derivation" implied some sort of reference to "matter of fact" of the impression order.

For these reasons, some of them (I think) solid, others (I allow) slighter or mostly presumptive, I am not inclined to accept what Mr. Kemp Smith and Mrs. Maund have said on this matter.

The alternative view, in Mrs. Maund's words, is that "'copy' must be interpreted in such a way that the copy implies no reference to its original" [M. 83].

As I understand Mrs. Maund and Mr. Kemp Smith, they want to hold that "meaning or reference" for Hume is extrinsic to ideas and properly applies to the "objects" or "accusatives" of belief and knowledge which are not impressions (unless one happens to be philosophising about impressions).

I shall not here discuss the large questions raised in this view of Humian "meaning or reference", but shall say something more about the non-referential interpretation of "copy".

Hume not infrequently speaks as if "the idea of x" were quite simply "an idea which is x-propertied". Thus we read at T. 240: "The very idea of extension is copied from nothing but an impression and consequently must perfectly agree with it. To say the idea of extension agrees to anything is to say it is extended." This *may* be simply the truism that a copy or replica must have the properties of what it copies, but it may be held to support Mrs. Maund's view. (And I suppose anyone might find some difficulty in distinguishing between a "blue image" and an "image of blue".)

Any such analysis would apply equally to ideas and impressions and so in terms of the analysis we should have to say that it is merely a trick of language that "idea of" is Humian English and "impression of" is not. I find this hard to believe.

Hume sometimes argues as if the "articles of inquisition" were only a device for obtaining clarity. A good example is the following [T. 72 f.]: "We need but reflect on that principle so oft insisted on, *that all our ideas are copied from our impressions.* For from thence we may immediately conclude that since our impressions are clear and precise, the ideas, which are copied from them, must be of the same nature, and can never, but from our fault, contain anything so dark and intricate. An idea is by its very nature weaker and

fainter than an impression; but being in every other respect the same cannot imply any very great mystery. If its weakness render it obscure 'tis our business to remedy that defect, as much as possible, by keeping the idea steady and precise."

I have tried to show that Hume usually meant something much more fundamental than a mere device (which could not be much more than a useful safeguard). What I have quoted from E. 22, T. 155, T. 64 f., T. 232, T. 251 seems to me either to imply or very strongly to suggest something much more drastic than a mere safeguard exuberantly (and arbitrarily) stated to be invariably requisite. But much in Hume, including several of the other passages I have quoted, could be read either way.

Hence I deserve censure for saying in my book on Hume (p. 32) that for Hume "ideas were essentially representative and were always felt to be so". At the very least I had insufficient evidence for the "always".

J. LAIRD.

VI.—CRITICAL NOTICE.

Plato's Theology. By F. Solmsen. Ithaca, N.Y.: Cornell University Press; London: H. Milford, 1942. Pp. ix + 201. 15s. 6d.

It is by a happy coincidence that Dr. Solmsen's scholarly and thoughtful study of Plato's theology appears almost simultaneously with Mr. Skemp's *Theory of Motion in Plato's Later Dialogues.* The two works, which have much in common, together form a most valuable corrective to the still not defunct misconception which sees nothing of capital importance in the Platonic philosophy besides the "Theory of Ideas" and apparently hardly knows of Plato himself except as the writer of the *Republic.* There are, of course, certain inevitable minor differences of point of view between the two scholars. Dr. Solmsen is clearly of opinion that the "theory of motion," which is at least as fundamental to a complete Platonism as the "Ideas" themselves, is most explicitly expounded in the tenth book of the *Laws,* whereas Mr. Skemp would refer us for it to the *Timæus* of which he regards the utterances of *Laws X* as only a "popularised" version, in the nature of what the French call an *œuvre de haute vulgarisation.* (My own opinion on the point, for whatever it may be worth, is, I own, rather on the side of Dr. Solmsen.) As, of course, also, the scope of Dr. Solmsen's study demands a consideration of something which the scope of Mr. Skemp's requires him to exclude, the "moral theology" of dialogues like the *Euthyphus* and *Republic II,* which is quite independent of the theory of motion characteristic of the *Phædras* and *Timæus* and *Laws.* But the main and vital point, in which both writers are at one, is that something more is, according to Plato, needed to account for both "natural" and "moral" order than the "Ideas," something which is as real as the "Ideas" and yet not itself an "Idea," and that something is a source of orderly motions which Plato calls sometimes *νοῦς* and sometimes a supreme *ἀρίστη ψυχή,* and which we should probably find it least misleading to call a wholly wise and righteous personality. The late Dr. Rashdall once complained, with some reason, that the neglect of the *Laws* by Oxford teachers of Greek philosophy had done much to blind their pupils to Plato's sincere and passionate "theism." If these teachers or their students will take the trouble to read these two excellent little books, the blindness, it may be hoped, will be pretty thoroughly cured.

Three of Dr. Solmsen's eleven chapters are given to a consideration of preliminaries, the close identification of the "gods" of the Greek city-state with the city itself and its interests and institutions, the effect of the "sophistic movement" in undermining this conception of the *Θεοὶ πολῖται,* and the various attempts we can detect in contemporary literature to arrest the decay of the simple-minded

earlier religious faith. What Dr. Solmsen has to say on these matters is, from the nature of the case, not particularly novel, but it is all attractively and freshly stated, and any comments I might be inclined to make upon it should be understood merely as a suggestion of qualifying reservations, not as indicating any serious disagreement in principle. I think, for example, that some reserves perhaps have to be made as we read Dr. Solmsen's statements about the supposed complete identification of the gods with the safeguarding of the social life of a particular city. Of course there is that traditional side to the Greek conception of divinity; we get it expressed most emphatically perhaps in the *Septem* of Æschylus. But I would suggest that there is another side to the picture. The major Hellenic deities seem never to have been thought of as bound up so *completely* with the life of the worshipping stock as Yahweh with that of the Israelites or Chemosh with that of Moab. We see this, for example, in the language which writers like Herodotus use when they describe the worships of remote and little-known peoples. It does not occur to them to say that of course the gods of these barbarians are "other gods" quite different from their own; they assume that these deities are of course the same beings as their own (the *real* deities), only called by other names and honoured with different rites. So at a later date, when Plutarch raises the question "who is the god of the Jews?" his answer is—"he is Dionysus, but they call him Iabe." We cannot easily imagine a Hebrew saying that "the Greeks worship Yahweh but call him Dionysus." There is, after all, from the first a potentiality of universalisation about a Greek god which you hardly find in a "Semitic" one. The great prophets, to be sure, reached the conclusion that Yahweh is the one and only God, but they did so, not like Greek philosophers, by saying that he is worshipped by all men under many names, but by declaring that the worship of all other men but themselves is wasted on the non-existent.

Another point on which I would suggest some possible reserve is how far the conception of the *Θεοὶ πολῖται* which we know to have been current at Athens can be assumed to have been equally prominent among all fifth-century Greeks. Can we be sure that in contemporary Ionia, where there was no longer any independent political life, the word *Θεὸς* carried the same implications as at Athens? It seems to me all the more dubious that, as we know, the Greeks themselves were puzzled about the etymology of the word. I call attention to this because in more than one passage Dr. Solmsen shows a tendency to assume a little too readily that the Ionian practice of calling a supposed primary substance or the *οὐρανὸς* itself *Θεὸς* implies what we should call a "religious" attribute towards the objects so denoted. I own that I do not feel convinced and am still tempted to agree rather with Burnet's view of what he calls the complete "secularism" of the "Ionian" outlook.

Perhaps I may take this opportunity also of mentioning another

XUM

point connected with the interpretation of Plato himself, where I think Dr. Solmsen would have been well-advised to tread cautiously. I think he reads into Plato *astrological* suggestions for which the dialogues afford no real justification. This is specially marked in what is said about the famous myth of the *Phædrus* (pp. 93-94), where it is taken for granted that the meaning of the procession of the Olympian gods in the myth is that each of these gods presides over one of the signs of the Zodiac, and consequently over the lives of all human souls which are connected with that sign. I really must ask whether there is any indication in the *Phædrus* or anywhere else in the dialogues that Plato attached any significance whatever to the Zodiac (which he never mentions), or supposed its signs to have any connection with the varieties of human character and fortune. I can see nothing in the *Phædrus* to suggest any real relation between the Olympians and the signs of the Zodiac beyond the mere fact that there happen to be twelve of each. But according to Plato's own myth, though there are twelve Olympians, only *eleven* of them ride in the procession; Hestia "stays at home." And the mere mention of the *twelve* proves nothing; it might readily be suggested by the simple fact of the existence at Athens of a κοινοβωμία of the δώδεκα θεοί. Before a Zodiacal astrology like that of Manilius is foisted on Plato, we ought at least to have some evidence that he recognized the existence of the twelve ζῳδία as a specially important class of constellations, and so far as I know there is *no* evidence at all. (Of course he talks in the *Timæus* about the *ecliptic*, the 'circle of the Other,' but that is a different matter. Similarly, Aristotle has much to say about the λοξὸς κύκλος, but never speaks, as the mediæval Aristotelians habitually do, of the *circulus duodecim signorum*.)

In the seven chapters devoted to the direct exposition of Plato the author distinguishes four different strands in Plato's theology. These are (1) the line of simple and straightforward ethical criticism of morally unworthy conceptions of the gods which culminates in the sweeping expurgation of mythology in *Republic II*, (2) the argument of *Phædrus* and *Laws X* with its conclusion that in the last resort *all* motion must be initiated by ψυχαί, the only agents which can genuinely *originate* movement, (3) the "teleological" argument, prominent in *Timæus* and in *Laws X*, from the regularity of the great astronomical revolutions to the universal supremacy of a wise and beneficent divine Providence, (4) the influence, which Dr. Solmsen rightly insists upon, on Plato's mind of the mystery-religions which taught the doctrine of an "hereafter" in which the individual reaps as he has sown. (Dr. Solmsen, I think, is fully justified in his contention that Plato's confidence on this point is not derived wholly from philosophical arguments like his own in *Laws X*: there is an element of antecedent *faith* in something not very different from what might be called authoritative revelation at the back of his mind.) The one criticism I should be inclined to make on these chapters and the three following ones in which we

are given the " comprehensive picture " of Plato's final doctrine of Soul, God, Providence and " Natural Law " is that Dr. Solmsen is perhaps a little too anxious to keep separate " lines of approach " to the problems which, as I conceive, were not really as sharply distinguished in Plato's own mind. Thus he tends to find a discrepancy between the *Timæus* and *Laws X*, and again, within *Laws X* itself, between the argument against the " atheist " based on Plato's analysis of motion and the assertion of " eternal Providence." His point is that the first line of thought conducts naturally and simply to the conception of an " immanent " soul of the cosmos as the source of movement, whereas the reasoning of the *Timæus* and the reply to doubts about Providence in *Laws X* imply the conception of a deity who " designs " the world from above and without, and is directly concerned with the moral state of the individual human soul. Is it then *ψυχή* or *νοῦς* which is Plato's deity ? Dr. Solmsen goes so far, in one place, as to say that the directing *νοῦς* which is the deity of the *Timæus* and of the second part of the theodicy of *Laws X* is a " different god " from the " soul " of the first part of the theodicy.

It must be admitted, no doubt, that Plato never gives a formal answer to the question raised here, whether his god is *ψυχή* or *νοῦς*. But did *he* for a moment dream of a possible separation of the two ? In the earlier part of the theodicy—as Dr. Solmsen himself seems to admit—it is never said that the *ἀρίστη ψυχή* which is in supreme control of the movements in the *κόσμος* is identical with the " cosmic soul " of the *Timæus*. If we are to ' harmonise ' the two dialogues, it would be equally possible to equate the *ἀρίστη ψυχή* of the one with the Demiurge of the other. Of course Dr. Solmsen's reply would be that this amounts to making not *ψυχή* but *νοῦς* your deity. But when we remember Plato's own expressed conviction that *νοῦς* can only exist " in *ψυχή*," I think we may fairly say that to him this is not a question of alternatives mutually exclusive of one another : where there is *νοῦς* there is *ψυχή* as its vehicle, and *e converso* an *ἀρίστη ψυχή* can only be one which is wholly and utterly " rational." And we might ask in just the same way what is the divinity of Shelley's *Adonais* or Wordsworth's *Tintern Abbey*, is it an immanent " soul of all things," or is it a " transcendent " spiritual principle of some kind, and I believe the true answer would be to say that for the poets this is not a matter of rival alternatives. Shelley, for example, would probably have said in words, if forced to say something, that what he worshipped was a " universal soul," but I do not think he would have thought for a moment that he was changing his belief when he also spoke of a deity " who made us for His ends and not our own." To find a conflict in principle between the account in the *Timæus* of the ' construction ' of the world by a Demiurge and the language of *Laws X* about the operation of the *ἀρίστη ψυχή* seems to me to be merely taking the consciously pictorial imagery of the *Timæus* with amazing literality.

XUM

Similarly, I do not myself see, as Dr. Solmsen apparently does, any inconsistency between the conception of God's "designing" activity in the *Timæus* and the teaching of *Laws X* about His concern with the moral state of individual human souls. It is true that in the *Timæus* we are *ex hypothesi* specially concerned with divine "prospective contrivance" as shown in the movements of the heavenly bodies and the construction of the corpuscles of bodies, and in the *Laws* with "prospective contrivance" as regulating the destiny of human persons in accord with their "deserts." But both are parts of one and the same Divine purpose. The concern of the demiurgic deity of the *Timæus* with the moral education of our souls is made plain enough by his instructions to the newly created souls at 41*d*-42*e*, not to mention numerous other references, and it seems a strange gloss upon *Laws* 904*a* ff., to take Plato to mean then that "our king," having once established by some sort of "first act" the principle that each soul "gravitates" to the society of morally like-minded souls, thereafter relapses into the aloofness of a deistic "great first cause." One might as well say that the God of orthodox Christianity is conceived as having arranged that He will "some fine day" have a final settling with His creatures, and then for the interim taking a Long Vacation from His functions as a Providence. The principle stated by Plato is, no doubt, simple enough, but it is the principle of what in execution would be an incessantly active and vigilant Providence.

With some such reserves as these I would heartily recommend Dr. Solmsen's study to the attention of all lovers of Plato, with a particular commendation of the valuable concluding chapter which deals with the influence of the Platonic theology on the thought of Aristotle, the Stoics and Origen.

A. E. TAYLOR.

VII.—NEW BOOKS.

Bradley's Dialectic. By R. W. Church. London: George Allen & Unwin, Ltd., 1942. Pp. 189. 10s. 6d.

In view of his eminence and his difficulty Bradley deserves a commentary as much as most philosophers of the remoter past, and one may well rejoice that Dr. Church has, after providing commentaries on Malebranche and Hume, turned his acute mind in this direction. However, the word *commentary* is perhaps hardly appropriate, for this book is not a general comprehensive discussion either of Bradley's work as a whole or of a single book of his but rather a selection of a few basic lines of thought to be found in him. The principles on which the author went in selecting what topics should be discussed are somewhat obscure; but the book turns chiefly on Bradley's view of relations, and its chief contribution is to present the latter in a more plausible form than has hitherto been done. This contribution is important. I think Dr. Church is successful in showing that the importance for Bradley's position of the argument from the infinite regress has been much overestimated, by myself among others, and that it is to be regarded simply as an argument against any view which treated relations as in no wise attributes of the related but as "independent" entities, and not as a basis of his whole position. It is an attack on one of the alternatives to Bradley's view, and as such is valid, for it is not Bradley but the view which he is criticizing that errs in treating relations as if they were not relations but terms, the usual ground on which the argument, when viewed as a general attack on relations, is rejected. The author discusses this question in connection with the ten senses of "internal relation" distinguished by the present reviewer in *Idealism;* and the chapter has at least convinced me that I have not presented Bradley's case adequately, though I think that if Dr. Church had taken adequate notice of the reservations I make in my book he would not have objected so strongly to me for saying that Bradley viewed relations as unreal. Bradley's issue about relations now seems to me to hang on two points. (1) Are we to take the notion of relation as indefinable and ultimate, so that to say things are related in a certain way is to say the most we can say by way of explaining their unity, or is a further explanation necessary? Bradley contends that it is necessary, since obviously a relation and the terms related are not merely a set of separate entities side by side but are abstractions from a unity, so that the antinomy arises that on the one hand relations must follow from the nature of the terms related and on the other we are unable to see how they can do so. To this an opponent may reply that the concept of relation is an unanalysable and ultimate form of unity, that in talking of matters of fact you are quite on the wrong tack if you ask for an explanation that would make them intelligible or logically necessary, and that we can see in particular cases how (though not why) particular relations constitute a unity; but even if one disagrees with such an answer it may still be objected that Bradley expressed the point very imperfectly when he concluded that all relational judgements are therefore "partly false" and infected with self-contradiction. To refer to facts which are unintelligible

XUM

to us is not to be self-contradictory or partly false. (2) Bradley maintains that the notion of merely numerical identity is senseless, and that from this it follows that you could never speak in any other than a comparative sense of the terms being the same if the relations were different; his opponents hold that numerical identity is an ultimate and irreducible fact. It would be well for philosophy to-day if Bradley were not just put aside but discussions on these very difficult questions resumed.

While the work of the author is intended to be and is mainly one of exegesis, in chapter viii on *Some Basic Difficulties* he passes to criticism and gives an alternative solution of his own to the problem of relations. "The disjunction between relations as separate, termless entities and relations as making a qualitative difference in their terms is vitiated", he thinks, "by the fact of an alternative which it does not exclude: namely, that on which relations make a difference in their terms that is not qualitative, but numerical merely".[1] The contention is rendered very difficult to follow when it becomes clear that by the terms of the relations are meant not the particular existents related as a whole but qualities. Whatever may be the case with things or events, how different *qualities* could be other than *qualitatively* different is not clear; but this criticism is perhaps only verbal since what he seems to mean is that different particulars may exhibit the same quality in different contexts (p. 149). However, it seems to me that his contention requires much more elaboration than it receives. He then, in the same chapter, proceeds to criticize Bradley on the ground that, even if we grant that the absolute truth is coherent, this does not provide us with a means of testing the degree to which the judgements actually at our disposal are coherent and so true.

There are two circumstances which render the book considerably less valuable than it might have been. In the first place Dr. Church is too fond of explaining Bradley in a terminology rather like his own. This is a mistake which it is very difficult to avoid but which with a difficult author possessing a background different from most philosophical students of the present day must render the work considerably less helpful. Too much use is also made of lengthy quotations. Secondly, the author does not seem to have profited by Bradley's work entitled *Essays on Truth and Reality*. The latter has much that is valuable to say on the topics discussed by Dr. Church, yet no reference is made to it whatever. As regards the subject-matter of his exegesis of Bradley the principal fault I have noticed is what seems to me his serious misrepresentation of Bradley's view of the law of contradiction.[2] He seems here to have read Bradley too much in the light of Hegel. Bradley does not deny the law of contradiction because identity and diversity are for him inseparable, but rather declares that because the inseparability of these incompatibles cannot be reconciled with the law of contradiction the relational way of thinking is self-contradictory and therefore partly false, and he makes the law of contradiction the fundamental law of Reality and as certain as any truth could be. I do not suggest of course that Dr. Church is unaware of the passages to which I have referred, but I do not see how he can reconcile them with his account of Bradley's attitude. I should also have very much welcomed a further elucidation of the sense in which relational judgements are held by Bradley to be partly false and of the very difficult notion of the removal of the contradiction in the Absolute. A further, general point of criticism I should like to make is that it would

[1] P. 145. [2] P. 22 ff.

have been more profitable if the author (and Bradley himself too, for that matter) had illustrated his argument constantly by reference to particular relations and not merely confined himself to talking about relation in general. After all the different kinds of "relations" are very different, and it may well even be the case that some of them are "relations" only in a very different sense from others so that no single theory of relations could possibly cover all. But despite these criticisms I certainly think the book ought to be read by all who want to study Bradley's thought at all thoroughly.

A. C. EWING.

The Dicts and Sayings of the Philosophers. The Translations made by Stephen Scrope, William Worcester and an Anonymous Translator. Edited by CURT F. BÜHLER. London: Published for the Early English Text Society by Humphrey Milford: Oxford University Press, 1941. Pp. lxviii + 408 + viii. 30s.

THE history of philosophical texts is an important part of the story of modern civilisation, and the history of this somewhat jejune collection of sayings from the wise men of antiquity is no exception to the rule. About the year 1063 of our era, an Arab philosopher living in Damascus named Abu'l Wefa Mubeschschir compiled a book of sayings of the philosophers, to which the title *Mokhtâr el-Hikam* was given. Although almost all his material was derived from classical sources, his work became a part of the wisdom literature of the Moslem world. During this period, Western Europe was beginning to awaken to the importance of ancient philosophy and culture, from which it had largely been cut off by the invasions of the Goths, the Huns and the Vandals, not to mention the schism between the Eastern and the Western Church; so that it was largely through Moorish channels that ancient classics like Aristotle first reached students in the west. The *Mohktâr el-Hikam* quickly attracted the attention of Spanish scholars, and between 1200 and 1250 it was translated into Spanish under the title *Bocados de Oro.* A Latin translation of this version called *Liber Philosophorum Moralium Antiquorum* appeared in the second half of the same century. A hundred years later Guillaume de Tignonville, Provost of the City of Paris, made a French version of this Latin translation, to which he gave the title *Dits Moraulx.*

Tignonville's rendering enjoyed a quite extraordinary popularity in Western Europe, and it was therefore only natural that several English translations were made in the course of the fifteenth century. Of these, the three most notable were by Stephen Scrope, Earl Rivers and an Anonymous Translator, whose work is preserved in a manuscript at Helmingham Hall. Rivers' translation was published by Caxton in 1477, the first English work ever printed; but the present editor has advanced conclusive evidence to show that all three versions were independently made and that Scrope's translation, which was completed in 1450, was antecedent both to the Anonymous Translation and to that of Rivers. He has also succeeded in identifying the group of French manuscripts from which Scrope made his translation. If he is right, the British Museum manuscript Royal 19 B IV, or some sister manuscript, was the source of Scrope's version. He has also identified the revised version of Scrope's translation, which is to be found in Emmanuel College Library (1. 2. 10), as the work of William Worcester. In addition, he has identified the

Abbreviated Version given in the Bodleian Library manuscript (Rawl. Poet. 32) as derived from a corrupt Scrope text found in the British Museum (Add. 34193). As translations, the Helmingham Hall version is much the best, and Scrope's text must be reckoned definitely inferior both to it and to Earl Rivers' version. As this last has been frequently printed, the present editor has contented himself with printing the Scrope text and the Helmingham Hall text on opposite pages, and giving the Abbreviated Scrope Version as an appendix. To this he has added an Apparatus Criticus giving the more notable variants in the English texts, and a series of notes in which Scrope's text is compared with the French and the Latin. No attempt has been made to comment on the contents of the work, nor to compare it with its Arabic original, partly because of the limitations of space, and partly because Dr. Margaret Schofield has dealt with some of these points in a dissertation on the Scrope version privately printed in 1936.

It goes without saying that an English translation of a French translation of a Latin translation of a Spanish translation of an Arabic text compiled from prior sources is not to be regarded as a serious contribution to philosophy; but it is of interest to observe what sort of work it was that won so much attention, before the printing press, in the sixteenth century, put the philosophical classics straight into the hands of scholars, and what aspects of classical wisdom succeeded in percolating through this curious filter. Primarily, the *Dicts and Sayings of the Philosophers* is a collection of brief sayings arranged under the head of the great names of Greek antiquity. Each of these is given a section to himself, and though some, like Hermes, are fabulous, and some, like Saquanwin, are unidentifiable except by conjecture, the majority of them are men whose personality as sages impressed the anthologists of Alexandria. Homer, Solon, Hippocrates, Pythagoras, Diogenes, Socrates, Plato, Aristotle, Alexander, Ptolemy, Galen and Protagoras are here, and in the case of men like Socrates and Alexander, who were notable in the field of action, as well as that of thought, considerable biographical details are given; though it is to be feared that the details are often adjusted to give additional point to the apothegms. There is at any rate a notable tendency for drama to outstrip history, as when an interview between Plato and Alexander is recorded. Very few Christians have managed to get into the text, and the few who have done so, like Gregory and Anselm, are probably there through pious mistranslations. So, despite the efforts of the Arabic encyclopædist, it is the personalities rather than the wisdom of antiquity that has survived through his *Mokhtâr el-Hikam*. Here and there a great saying has come through intact, such as that attributed to Aristotle by Diogenes Laertius. 'Thanne it was asked him what maner of mene shulde be called frendis; he aunsuerd and seide: thei that have but oon soulle in divers bodies' (p. 65. See Diogenes Laertius, v. 20, under Aristotle). Socrates, Plato, Aristotle and Alexander occupy more than a half of the text, and it is easy to see that it was Alexander's connection with the philosopher that won him this important place. In short, it seems that posterity thinks very highly of philosophers, provided they prove themselves also to be men of action.

As regards the general tone of this work, which impressed itself so much on the minds of our ancestors as to be the first book printed in the vernacular, it is curious to note how perfectly it accords with the spirit of the later Middle Ages. In the first place, the *Dicts and Sayings of the Philosophers* is a book of moral philosophy; it has little to say about

nature and very little about God, except as the moral ruler of creation. In the second place, its conception of virtue is of the stoic rather than the heroic kind. Patience, moderation in desire, honesty and fair dealing are the clues to happiness. 'And thei asked him what thynge sustened trouthe; he ansuered and seide: witte. And thane ho gouerneth witte? He seide: the kepynge of a mannes tunge. Ho kepeth the tunge? Pacience. Ho kepeth for to have pacience? The drede of God. And what maketh a man to drede God? Spekinge of the dethe and to knowe his freelte. And anothire seithe: superfluytee maketh the body seeke. the wyne troubleth the witte, wrathe is contrarye to sapience; but attemperaunce comforteth the herte, it putteth awey heuynesse and sendeth heele' (p. 285). Thirdly, there is reflected in it something of that love of chivalry which existed side by side with this cautious attitude in the mediæval mind. Alexander's generosity to the conquered Darius, respect for a king who thinks of the prosperity of his subjects, such things recur time and again, and reflect another not less important aspect of the work. 'And he saythe that the peple ys happy when they have a kynge of guod discrescyon, of guode cownsell, and wyse yn scyens; and the peple ys full vnhappy when any of thys forsayd thynges faylythe yn there kynge' (p. 297).

It only remains to add that the three texts have been edited and annotated with great fidelity and industry, and that there is little doubt that they will contribute greatly to our knowledge of English palæography and linguistic usage. Occasionally the editor slips in his use of current English, as in the footnote to page lxiii, where he uses the word 'connotations' instead of the simpler 'meanings'; and anyone familiar with the work of the best modern palæographers cannot but feel that his inferences regarding the manuscript tradition, though undoubtedly correct and based upon good evidence, are clumsily expressed and rather badly arranged. Yet it is to be hoped that Mr. Bühler's prodigious industry and absolute honesty with his readers will receive from the learned world the recognition that they undoubtedly deserve.

ARTHUR T. SHILLINGLAW.

Truth and Fallacy in Educational Theory. By CHARLES D. HARDIE. Cambridge University Press, 1942. Pp. x + 151. 6s.

A VERY great proportion of the work on educational theory in recent years has been done by experimentalists who have been more interested in accumulating facts than in thinking clearly about them. Mr. Hardie has set out to perform the very necessary task of applying a mind trained in logical analysis to correcting the ambiguities and fallacies which have found their way into this subject. He also attempts the more ambitious task of laying down, as a result of this analysis, the fundamental framework of any sound educational theory.

The first of these tasks certainly required doing; it is more doubtful whether we are yet in a position to perform the second. If sometimes Mr. Hardie seems to advance criticism to which the experimental educational psychologist might find a reply which the author has not considered, the book is nevertheless to be welcomed as an honest attempt to remove confusion and one which may fulfil part of its function by arousing controversy in which doubtful issues may be clarified.

While the author's criticism is often acute and generally just, he seems

sometimes to fall into the error of supposing that to convict an educationalist of muddled thinking is the same as to convict him of being wrong in his conclusions. Professor Dewey, for example, gave what appear to most of us to be foolish reasons for considering that children should find out things for themselves, and Mr. Hardie does excellent work in clearing away a mist of muddled thinking from this topic. It may, however, remain true that the best way of teaching some subjects is not to tell children the answers but to let them find out for themselves. Such a teaching method may be adopted for the sensible reason that it is found to be the best way in which such facts can be learned and remembered and not for the foolish reason that it is the way that recapitulates the history of discovery.

A certain amount of the impatience with which experimentalists treat the efforts of philosophers to clarify their thinking is due to the fact that these seem not always to know sufficiently well the experimentally ascertained facts or to appreciate the experimental point of view. A certain degree of failure to appreciate the experimentalist's point of view appears in the author's discussion of intelligence tests. He says: "It seems to me to be quite possible that I could obtain as high a score as Professor Einstein could in CAVD [Thorndike's early set of intelligence tests]. Psychologists, when confronted with such an objection as this, generally say that Einstein's outstanding achievements are not perhaps due to an intelligence in excess of mine but . . . ." Intellectual debate is too easy if one is permitted to invent one's opponent's answers to objections. What the experimental psychologist would really say is that whether Mr. Hardie or Professor Einstein would do better on an intelligence test is a question of fact to be decided by making the experiment. The required experiment does not seem to have been performed and Mr. Hardie's suppositions about its probable result are not evidence and, therefore, call for no explanation.

That a philosopher should set himself to rebuke the muddled thinking of educational psychologists is an excellent thing, and the rebuke is often deserved. It is necessary, however, that one who undertakes this task should be scrupulously exact in his own thinking. Mr. Hardie is not always as careful as he should be. On page 32, for example, he argues against intuitive ethical theory: ". . . it appears that one person might, by insight, know that a certain act was right, and another person might, by insight, know that the same act was wrong, and if there is no method of resolving the disagreement then controversy on moral questions would appear to be a waste of time. But since people evidently do not believe such controversy to be waste of time it would seem that this theory must be mistaken." The logically justifiable conclusion would seem to be that either people do not believe the intuitive theory or else that they do not understand that the theory implies that moral controversies are a waste of time. That "this theory must be mistaken" is evidently not a conclusion which follows from the premisses stated.

Insufficient knowledge of recent experimental work is most apparent in the fourth and most ambitious chapter, on 'The Basis of any Educational Theory'. Certainly Mr. Hardie's basis is one which any up-to-date experimental psychologist would unhesitatingly reject. The author's view that learning is either learning by trial and error (Thorndike) or the formation of conditioned reflexes (Pavlov), with insight as a doubtful third method probably reducible to one of the other two, harks back to simple behaviourism at its worst. Since J. B. Watson won widespread credence

for such mechanistic theories of learning twenty years ago much has happened which should be appreciated by anyone trying to construct a basis for education. Koffka's criticism of Thorndike's formulation has shown that the facts of learning on which the theory of trial and error learning was erected are not to be explained without assuming insight. Köhler's brilliant experiments on chimpanzee learning have opened a new field in the study of animal learning and have led to the ingenious experiments of Adams which have shown that even cats learning to open puzzle-boxes like those of Thorndike's original experiments do not learn in the simple mechanical way that Thorndike supposed. Now that experiment has shown that simple mechanical theories of learning are inadequate to explain even animal learning, it is unreasonable to ask us to accept such theories as the basis for human education. Educational theory will not find its rational basis in the discarded theories of the early experimenters on animal learning.

The fact that much is found to criticise in this book does not mean that it has not also merits which make its publication welcome. It is clear, honest, and provocative. If it provokes experimental educationalists to consider its criticisms, to profit by them when they are sound and to refute them when they are not, it may be of great service in clarifying thought on this subject.

R. H. THOULESS.

Received also :—

A. H. Smith, *A Treatise on Knowledge*, Oxford, Clarendon Press, 1943, pp. vi + 182, 10s. 6d.

J. C. Cooley, *A Primer of Formal Logic*, New York, The Macmillan Company, 1942, pp. xi + 378, $3.00.

Ganganath Jha, *Pūrva-Mīmāṁsā in its Sources*, Benares Hindu University, 1942, pp. xx + 386 + viii + 81, 15s.

E. L. Minar, Jr., *Early Pythagorean Politics in Practice and Theory*, Baltimore, Waverley Press Inc., 1942, pp. 143, $2.00.

A. Dulles, *Princeps Concordiae : Pico della Mirandola and the Scholastic Tradition*, Cambridge, Mass., Harvard Univ. Press ; London, H. Milford, 1942, pp. 182, 11s. 6d.

H. F. Stewart, *Blaise Pascal* (Annual Lecture on a Master Mind, British Academy, 1942), London, H. Milford, 1942, pp. 20, 2s.

A. A. Luce, *The Purpose and the Date of Berkeley's "Commonplace Book"* (from the Proceedings of the Royal Irish Academy, Vol. XLVIII, Section C, No. 7), Dublin, Hodges, Figgis & Co., 1943, 1s.

Hegel's Philosophy of Right, translated with notes by T. M. Knox, Oxford, Clarendon Press, 1942, pp. xvi + 382, 21s.

Schelling's The Ages of the World, translated with introduction and notes by F. de W. Bolman, Jr., New York, Columbia Univ. Press ; London, H. Milford, 1942, pp. xi + 251, 20s.

F. W. Jones, *Habit and Heritage*, London, Kegan Paul, Trench, Trubner & Co., 1943, pp. 100, 5s.

P. Schilder, *Goals and Desires of Man : A Psychological Survey of Life*, New York, Columbia Univ. Press ; London, H. Milford, 1942, pp. xii + 305, 26s. 6d.

P. Schilder, *Mind : Perception and Thought in their Constructive Aspects*, New York, Columbia Univ. Press ; London, H. Milford, 1942, pp. xii + 432, 33s. 6d.

S. Lowy, *Foundations of Dream-Interpretation*, London, Kegan Paul, Trench, Trubner & Co., 1942, pp. xvi + 260, 16s. 6d.

F. L. Burdette (ed.), *Education for Citizen Responsibilities*, Princeton Univ. Press ; London, H. Milford, 1942, pp. xiii + 126, 10s.

C. L. Burdick, *As I See Life*, Glasgow, W. Maclellan, 1942, pp. 94, 5s.

J. Lyth, *Marcus Aurelius : A Rendering in Verse*, London, G. Allen & Unwin, 1942, pp. 29, 2s.

A. L. Macfie, *Economic Efficiency and Social Welfare*, London, H. Milford, 1943, pp. xi + 142, 10s. 6d.

J. Needham, *Time : The Refreshing River*, London, G. Allen & Unwin, 1943, pp. 280, 16s.

A. J. Peaslee, *A Permanent United Nations*, New York, G. P. Putnam's Sons, 1942, pp. x + 146, $1.50.

T. C. Pollock, *The Nature of Literature*, Princeton Univ. Press ; London, H. Milford, 1942, pp. xxiv + 218, 20s.

I. A. Richards, *How to Read a Page*, London, Kegan Paul, Trench, Trubner & Co., 1943, pp. 246, 10s. 6d.

E. G. Suhr, *Two Currents in the Thought Stream of Europe*, Baltimore, The John Hopkins Press ; London, H. Milford, 1942, pp. 469, 30s.

Proceedings of the British Academy (1940), London, H. Milford, 1943, pp. 530, 30s.

VIII.—PHILOSOPHICAL PERIODICALS.

PROCEEDINGS OF THE ARISTOTELIAN SOCIETY. N.S.: Vol. xli, 1940-41. **H. D. Oakeley.** The Presidential Address: *How is History Possible?* ['There could be no history without the meaning which is brought to the otherwise meaningless process of things, by something which is within it, but also beyond—namely the subject of experience. . . . The whole conception of history which I am attempting to put before the society, postulates that in some sense there is in human selves a creative capacity in respect to the introduction of a higher value into the conditions of existence. . . . The freedom which a society in any one present may claim to transform its own character in any important respect is or may be narrowly limited by the fact that it cannot convey its ideals and the will dependent on them to its successor.'] **D. M. Mackinnon.** *What is a Metaphysical Statement?* [The paper has in view recent writers hostile to metaphysics—phenomenalists and those who practise 'analysis'. The author is not prepared to defend idealistic monism against their criticism, but he contends that a pluralistic metaphysics, in which the problem of the self is central, has, by reason of its greater readiness to accept facts and to recognise ultimate and irreducible types of fact, a greater claim to consideration.] **D. M. Emmet.** *The Use of Analogy in Metaphysics.* [The analogy with which the paper deals is not the analogy of the logic books, but inference from some part or feature of experience to the nature of the ultimate reality. Such inference may, the writer thinks, be disciplined by logical and scientific criticism, while it looks to poetic and religious imagery to enrich its content. But "metaphysical analogies still remain highly selective, and come back in the end to the metaphysician's own personal impression of what is significant".] **F. J. O. Coddington.** *On Thinking about the Universe.* [The writer warns us that the paper 'was originally written for a small group of personal friends', and must therefore be taken as a 'confession of faith' rather than as a piece of 'closely-knit argument'. The first part of the paper dwells on the hindrances involved in the words and ideas which are the instruments of our thinking, and then speaks of our thinking itself, which, in the writer's view, 'is actually done by our subconscious'. The paper then goes on to sketch the results of the writer's own reflections about himself and the universe. Some of these reflections are rather crude, as when he says, 'No man gets outside his eye, his ear, his skin', and compares the subject in relation to his brain to the 'telephone operator seated in a telephone exchange who has never been outside that exchange'. He thinks that the independent existence of matter can best be conceived by attributing to matter an aspect of 'psychic internality', and as regards the universe has always 'the mystic sense of life, energy, psychic activity struggling upward to light, to thought']. **D. G. C. Macnabb.** *Phenomenalism.* [The aim of the paper is to defend phenomenalism against the sort of objections which were raised by Stout in a paper read to the Society in a previous session. A number of objections are stated; the discussion of them is rather wandering and not easily summarised. The following quotation may serve to indicate both the writer's general position and its weakness. 'It is one thing to state that a material [thing] exists, another to state that it is observed or not observed. And the meaning

of the former statement must be the same whether we add that it is observed or that it is not observed. . . . This is the point Berkeley failed to notice.' What the writer fails to notice is that statements about a material thing which mention 'only sense experience' cannot be true of material things of which at the time there is no sense experience. Stout's criticism of 'permanent possibilities of sensation' is final so far as strict phenomenalism is concerned.] **M. Macdonald.** *The Language of Political Theory.* [The writer candidly tells us that she knows 'little about either history or politics', but she has 'recently been forced to read a fair amount of what is called political theory or political philosophy', and the present paper is apparently the outcome of these enforced studies. If one is to imitate her own candour, one must say that she does not appear to have profited greatly from them. She criticises the theories of the Social Contract, the General Will, and Utilitarianism, and particularly the terminology in which the first two are expressed, and thinks that they all 'imply that we can know once and for all almost by learning a single sentence, how and when political obedience is justified'—as if writers like Mill, Bosanquet, and Sidgwick would devote laborious discussions to matters that could be stated in 'almost a single sentence'. The writer thinks 'there is no general criterion, but an indefinite set of vaguely shifting criteria', but such a widespread fact as that of political obedience must surely have a correspondingly general explanation.] **L. A. Reid.** *Knowing in Music.* [The paper begins by enumerating the main elements of music on which musical effects depend. After discussing a few of these shortly, it goes on to speak of the experience of music in its emotional aspect, and again, as an experience of significance. At this point the distinction between pure music and music that is representative is referred to, and finally various types of listening are noticed.] **O. Neurath.** *Universal Jargon and Terminology.* [Neurath proposes to call studies that deal systematically with expressions and terms 'Terminology', and the intention of his present paper appears to be to give specimens of the way in which 'Terminology' would deal with various types of terms. Sections of the paper have such headings as 'Dangerous Terms', 'Value Terms', 'Causation Terms'. To readers who are acquainted with Neurath's theoretical views the paper will no doubt have its interest, but readers who are not are unlikely to derive much instruction from it.] **J. W. Harvey.** *Knowledge of the Past.* [In the first part of his paper the writer contends that philosophy has not dealt adequately or satisfactorily with our knowledge of the past, and illustrates his contention by reference to Descartes, Kant, and J. S. Mill. In the second part he first affirms that we may be certain that we do possess both knowledge of the past generally and memory knowledge in particular, even though we may be mistaken in particular cases. He then goes on to discuss two questions regarding memory knowledge, *viz.*, whether it is immediate and what part is played by the memory-image. He considers the views of Broad and Stout on these questions and finds them not wholly satisfying. He is inclined himself apparently to fall back on the uniqueness of memory as a faculty.]